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VOLUME VI

1932

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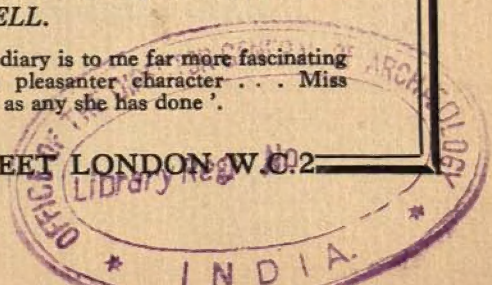
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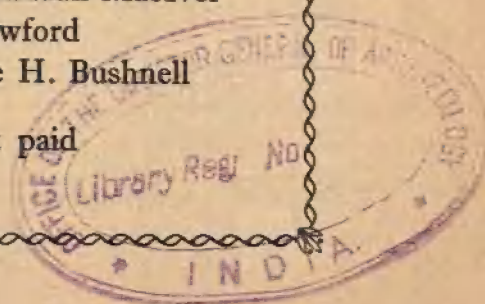
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LONDON: MACMILLAN & CO., LIMITED
NEW YORK: THE MACMILLAN COMPANY

Antiquity

A Quarterly Review of Archaeology

VOL. VI No. 21

MARCH 1932

Editorial Notes

ARCHAEOLOGY is still something of a Cinderella at the British universities. Certain branches of this vast subject are indeed taught (for the most part incompletely) at a few of them: notably, classical archaeology in Oxford, Cambridge and London; Egyptology, more particularly at the first and last; the prehistory of Europe and the Near East at Cambridge and Edinburgh; British archaeology in London and at Cardiff; Romano-British archaeology at Durham and Newcastle. But all this is both very scattered and very restricted. When we remember that problems of first-class importance are now being attacked by British archaeologists in Greece, Palestine, Egypt, Irak, India, China, South America, South Africa, Kenya, and even in Great Britain itself, we realize that the facilities for preliminary training and subsequent research offered by our British universities are inadequate to the point of the ridiculous. And to the sum-total of professional archaeology must of course be added that incidental contact with archaeological materials or problems which from time to time complicates the routine of Dominion and Colonial administration. Some regularized instruction in the archaeology of our Colonies might (if the necessary machinery were available) reasonably be added to the burdens of our young Colonial civil servants. But the machinery is, in fact, *not* available; or, rather, no determined attempt has yet been made to set up the machinery from the parts that

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exist. There is nothing of that reasoned cooperation without which economy of effort and speedy and effective development cannot be expected. The time for cooperation and concentration is overdue.

❧ ❧ ❧

One attempt was, it is true, made on a small scale before the War. At Liverpool an Institute of Archaeology, with three or four Chairs, was established as a focus for teaching and research, and has produced some notable work. But both the place and, perhaps, the time were ill-chosen. Liverpool has many claims to distinction, but it cannot claim to possess any special qualifications as a centre for the study of the world's archaeology. Even Oxford and Cambridge have not, and can scarcely hope to have, all the materials necessary for an Archaeological Institute on a really comprehensive scale. London stands—potentially—alone in this respect. Only in London can we expect permanently that convergence of materials and workers which alone can make such an Institute a real, world-wide power in this department of science. And, translated into academic terms, this is as much as to say that the responsibility for taking the greatest step ever yet attempted in the administration of archaeology, on an international basis, devolves now upon the University of London.

❧ ❧ ❧

The University of London is a century old, but is only now in a position to put its house in order. For it is indeed only now, at long last, on the point of owning a house to put in order. Hitherto the University has been a lodger on remote and alien ground. Its new buildings, when they arise on the northern side of the British Museum, will be its first real home, and will at once present it with facilities for development that few, if any, other universities in the world can rival. How far it will take advantage of these facilities remains to be seen. In the meantime, those University departments of knowledge which rejoice in far-seeing directors and deep pockets are staking their claims for permanent accommodation. The Institute of Historical Research, established a few years ago on the initiative of Professor A.F. Pollard and already a flourishing centre of postgraduate work, will vacate its hutments and clothe itself in bricks and mortar. The Institute of Art, recently founded through the munificence of Mr Samuel Courtauld, will begin its labours under Professor W. G. Constable.

EDITORIAL NOTES

It remains to add the third member of the trinity, and to establish, in close juxtaposition to these two Institutes, that Institute of Archaeology which would inevitably share so closely in many of their needs and interests.



In this special context, the whole matter is worthy alike of earnest thought and urgent action. Here is an unrivalled chance for co-ordination not only within the limits of archaeology itself but also between archaeology and two kindred departments of knowledge. Take the case of the libraries of these various establishments. The overlaps between archaeology, history, and the history of art are multitudinous: what economy of space and expenditure to have them side-by-side! A small scientific laboratory for research work and instruction in preservative measures—a necessary element in the training of every field- or museum-archaeologist—is equally necessary to archaeology and to art. And so on. It is unnecessary to elaborate the essential affinity of interest between the three Institutes. With this and other factors in mind, the University Board of Studies in Archaeology has prepared a carefully considered scheme for an Institute of Archaeology which shall occupy a part of the top floor of the new University buildings, alongside the Institute of Art. The scheme provides for the close storage of teaching and research-collections of a kind nowhere at present available, and includes the mechanism necessary for postgraduate work in all branches of archaeology.



To the details of this scheme we shall return in a future issue. It is a scheme which is both practical and urgent. But the most urgent part about it is that *now or never* must it find a sufficient financial guarantee to secure from the University of London the required minimum accommodation. This accommodation—which is, be it emphasized, the essential foundation of the whole scheme—will cost £28,000. The sum is large, but in no way disproportionate to the magnitude of the development which it represents, and it is in fact considerably less than is being spent on the Institute of Art. Under other conditions, isolated from the kindred institutes and the British Museum, such an Institute of Archaeology would cost incomparably more and would be incomparably less effective. But the plans for the

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new University buildings are already under preparation, and time is short. Unless a substantial part of that £28,000 is guaranteed almost immediately, we shall have missed perhaps the greatest opportunity that organized archaeological research has ever had in Europe. Who will help?



The Editorial notes on our domestic affairs, printed in the December number, have met with an encouraging response, and we have received many friendly letters from subscribers, some of whom were induced by our 'appeal' to change their intentions. Others have told us they cannot do without ANTIQUITY—it was described by one as an 'educational luxury'—and will continue it in spite of the difficulties in which we all share. Some, of course, are compelled to withdraw their support—only temporarily we hope—but at the same time express their great regret at having to do so. We also wish to thank those who have sent their subscriptions for 1932; the next paragraph is inserted for the attention of those who have not yet done so.

The SUBSCRIPTION to ANTIQUITY for 1932 is now DUE. We would remind our Subscribers of the form and envelope inserted in the December number for the purpose of remitting payments. *An early response will be much appreciated as this will save avoidable trouble in having to send out direct reminders.*

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The Etruscans and Pompeii

by R. C. CARRINGTON

THE evidence for Pompeian history has been furnished almost entirely by the use of the spade, and, while for the latest period its full and detailed character makes it difficult to pick out a unifying thread through it all, the further back in time one goes the scantier direct testimony becomes ; until, in dealing with the earliest periods, we are, as it were, building up a case out of evidence which is largely ' circumstantial '. This defect is inherent in the archaeological study of any site, since it is to be expected that what was built latest should be best preserved, but the defect has been aggravated at Pompeii by the way in which, until recently, the excavations have been carried out.

The confessed object of these excavations has been not to trace the development of the town from the beginning to the end of its history, but to preserve and present to the visitor's eyes a picture of Pompeian life as it appeared at one definite period—the few years immediately preceding the eruption. Thus, as soon as the latest level was reached, the digging stopped, and no attempt was made to penetrate to the lower and earlier strata. The results, though interesting for the tourist, were unsatisfactory for the historian. In the last few years, however, deep excavation has been carried out at various places on the line of the city-walls and within the area of the town, and this, happily, is said to be but the prelude to further digging of a similar type. Deeper excavation will solve many problems which at present we can only attempt to solve by conjecture or inference. One of the most important of these problems is that of the part played by the Etruscans in the growth of the town, interest in which has been recently renewed by the publication of the results of the excavations made on Pompeii's city-walls.¹ Hitherto, discussion of this question has been a little unreal, since, with the exception of a passage in Strabo which will be

¹ Amedeo Maiuri, *Studi e Ricerche sulla Fortificazione di Pompei*, in *Monumenti Antichi*, 1930, vol. XXXIII, part 2.

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mentioned later, the data requisite for discussion, *viz.*, those furnished by archaeological evidence, were lacking.

Before the recent excavations were begun, it was recognized that the system of fortification, as it existed at the time of the eruption of A.D. 79, contained work of at least three different epochs. The latest stage in its history, consisting in the abandonment of its use for military purposes and in the partial building-over of the line which it had occupied by private houses, was recognized to have been a development of the early decades of our era. The next stage (working backwards in the chronological series), which consisted in the insertion of square rubble-work towers at intervals in the wall and the reconstruction of large stretches of the wall itself in the same material, was correctly dated to the beginning of the first century B.C., to the period, that is, of intense preparation which preceded the final struggle between Rome and her Italic allies over the question of the franchise. Beyond this nothing certain was known, and the earlier stages of development were wrapped in darkness. The new excavations, however, have shed an entirely fresh light on the origin and history of these walls, and have furnished us with our first tangible evidence for testing the truth of the various views which have been held by ancient and modern writers on the growth of the town.

Signor Maiuri, the Director of the Naples Museum, who was in charge of the excavations, has traced the following five periods in the development of Pompeii's fortifications :—

(1) 520–450 B.C. (the period of their original construction). The fortification, which may have been 18 feet high on its outer face, was composed of a double curtain of limestone ashlar masonry (fig. 1, A–A') containing an inner core of beaten earth (a). The courses of the ashlar masonry were very irregular in width (plate 1).

(2) 400–300 B.C. The outer curtain of the wall was demolished, and in its place was erected a new curtain about 30 feet high, of limestone ashlar masonry, differing from the one it replaced in the greater thickness of its blocks and in the regularity of the width of its courses (fig. 1, B). Inside this was piled a sloping *agger* (b), with a low revetment on its inner face (B'). The *agger* completely covered the inner revetment of the original wall.

(3) 300–180 B.C. The outer curtain (B) had its height increased (c), and in places was entirely replaced, by a construction in ashlar masonry of volcanic tufa. At the same time, and in consequence of

THE ETRUSCANS AND POMPEII

the increase in the height of the outer facing, the *agger* was widened (c-c'). Moreover, the platform on the top of the fortification, on which the defenders stood, was strengthened by the construction in the core of the *agger* of an inner wall of ashlar masonry, mostly of tufa (c'').

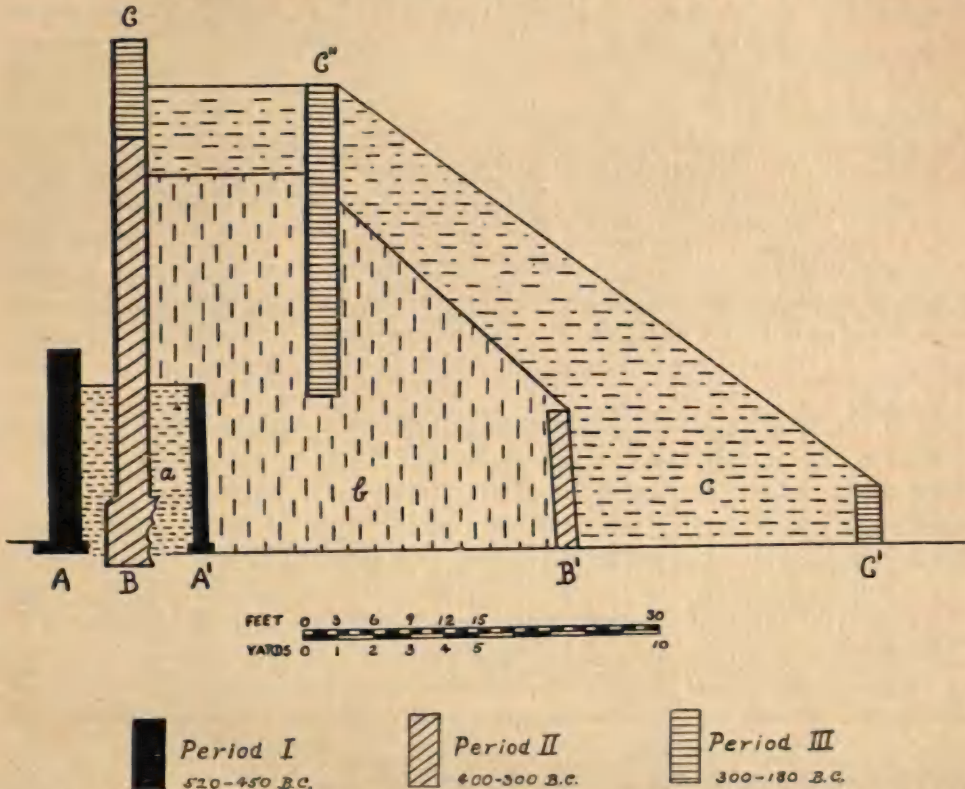


FIG. 1. Cross-section of the Pompeian fortifications of periods I, II, and III, reconstructed according to data furnished by the report of the excavation

(4) 120-89 B.C. The ashlar masonry of the outer facing was largely replaced by rubble-work of lava. Square towers were inserted at intervals, occupying the space between the outer curtain (c) and the inner wall (c'').

(5) 80 B.C.-A.D. 79. The military use of the fortifications, as has already been stated, was gradually given up.

The walls are believed by the excavator to have been constructed originally (period I) by the Oscans, working under the direct influence

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of the Greek colonies of Cumae, Neapolis, and Posidonia (Paestum), and the reason he suggests for the action of the Oscans is a desire for safety against the attacks of the Etruscans from the interior of Campania. Periods II, III, and IV were due to the Samnites, who, as is well-known, invaded Campania at the end of the fifth century and remained dominant in Pompeii until the Dictator Sulla planted a colony of Roman veterans in the town. Period V is subsequent to the founding of this colony.

It is no part of the scope of this paper to consider all these five periods in detail. Only the first of them (A, a, A') directly concerns us, since the other four all belong to a time when the Etruscan domination in Campania had long been shattered. Two other points, however, have to be mentioned in order to throw into full relief the great importance of the discovery of a wall which dates precisely from the time when the Etruscans were present in the region of Pompeii. In the first place, there is little doubt that the original nucleus from which Pompeii developed was a village, to which the greater part of the present city was a later addition (fig. 2).² The original village comprised the area of the Forum and the adjoining *insulae*, while the spur of land on which the sixth-century Doric temple stands served as acropolis. The later town was bounded by the walls which we are discussing. It follows, therefore, that the people who erected the fortification of period I were the people who enlarged Pompeii from a village to a town. In the second place, Strabo definitely states that the earliest inhabitants of Pompeii were Oscans, who were succeeded by the Etruscans and Pelasgians, and these in turn by the Samnites.³ The purpose of this paper is to show that there is strong reason for thinking that the work of the Etruscans in Pompeii consisted simply in enlarging it from a village to a town, and that the tradition which Strabo has preserved is based on solid fact. The proof of our thesis will depend entirely on the coherence of circumstantial evidence, since it must be admitted at once that among the rare objects found during the excavation nothing was discovered which can be connected with the Etruscans directly and beyond possibility of doubt. It will be convenient to approach the problem from three different angles, considering (1) various scraps of evidence (other than that of the town-wall) which suggest that the enlargement of Pompeii was the work of the Etruscans, (2) the precise nature of the town-wall of period I, especially in comparison with the

² F. Haverfield, *Ancient Town-Planning*, pp. 63-9; v. Gerkan, *Griechische Städteanlagen*, pp. 119-120.

³ v, p. 247.

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walls of Posidonia, (3) certain coin-series prevalent in South Italy during the sixth, fifth, and fourth centuries B.C., which enable us to supply a motive for the action of the Etruscans in planting a strong settlement on the site of Pompeii at the end of the sixth century.

Archaeological discoveries, such as the inscribed tile from Santa Maria di Capua,⁴ the archaic terracottas found in the same place and elsewhere in Campania, which bear a close resemblance to terracottas of admittedly Etruscan origin,⁵ and the early Campanian inscriptions which seem to be written in a mixture of Etruscan and Oscan,⁶ confirm the view handed down to us by several ancient writers,⁷ but denied by some modern scholars, that Campania came within the sphere of Etruscan conquest: and research into the art and language of South Italy gives results which are difficult to explain if the dominion of the Etruscans did not at some time extend even further south than Campania.⁸ If an Etruscan conquest of Campania be accepted, it follows *a fortiori* that Pompeii came within the sphere of their domination. Apart from this, the detailed evidence relative to the question is as follows:—

(1) The plan of Pompeii is purely Italic. The town comprises, as has already been mentioned, the area of an original village and a later accretion. Each of these distinct portions of the town is laid out round two main axes—a *cardo* and a *decumanus* (fig. 2). The *cardo* of the original village is preserved in the present Strada del Foro and Strada delle Scuole; its *decumanus* in the Strada della Marina and the western portion of the Strada dell'Abbondanza. In the enlarged town, the Strada Stabiana represents the *cardo* and the Strada Nolana the *decumanus*. The occurrence of two main axes, both in the original village and in the later town, shows that the Greek method of laying out a town with more than one *decumanus* and *cardo* of equal importance had no influence here and that the origin of the Pompeian plan must be sought on Italic soil.

⁴ Trombetti, *La Lingua Etrusca*, p. 141 ff.

⁵ Koch, *Dachterrakotten aus Campanien*.

⁶ Pauli, *Altitalische Studien*, vol. III.

⁷ Polybius, II, 17, 1; Velleius Paterculus, I, 7; Strabo v, p. 242; Dionysius of Halicarnassus, VII, 3.

⁸ V. Macchioro, *Gli Elementi Etrusco-italici nell'Arte e nella Civiltà dell'Italia Meridionale*, in *Neapolis*, I, 1913, pp. 270-306.

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(2) The shapes of the *insulae* which resulted from this town-planning are in the main not rectangular, like those of a Greek city, but trapezoidal (fig. 2). 'Neither its oblongs, nor its squares, nor its street-crossings exhibit true right angles, though many of the rooms and peristyles in the private houses are regular enough. In this feature Pompeii resembles the trapezoidal outlines of the Terremare. It resembles also much Roman military work, both of Republican and of



FIG. 2. Street-plan of Pompeii, showing the area of the original village, and (in red) the main axes of this village and of the enlarged town

Imperial date, which disregards the strict right-angle, and accepts squares and oblongs which are, so to say, askew . . . Whatever the reason, the trapezoidal house-blocks of Pompeii exhibit a feature which is not alien to the earlier town-planning of Italy, though it is strange to the cities of Greece'.⁹

(3) Ancient writers record various peoples as inhabiting the southern and eastern portions of Campania, and in some way or other they can all be connected with the Etruscans. Polybius says that these regions

⁹ F. Haverfield, *op. cit.*, pp. 67-8.

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were occupied by the Daunians and Nolans.¹⁰ Of these two peoples, the Daunians are mentioned by Dionysius of Halicarnassus as having taken part with the Etruscans in an expedition against Cumae in 524 B.C.,¹¹ while the territory of the Nolans is said by Polybius to have been conquered by the Etruscans at the same time as the valley of the Po (*i.e.*, the end of the sixth century B.C.).¹² The elder Pliny mentions the Umbri as amongst the peoples that ruled over Campania,¹³ and they,

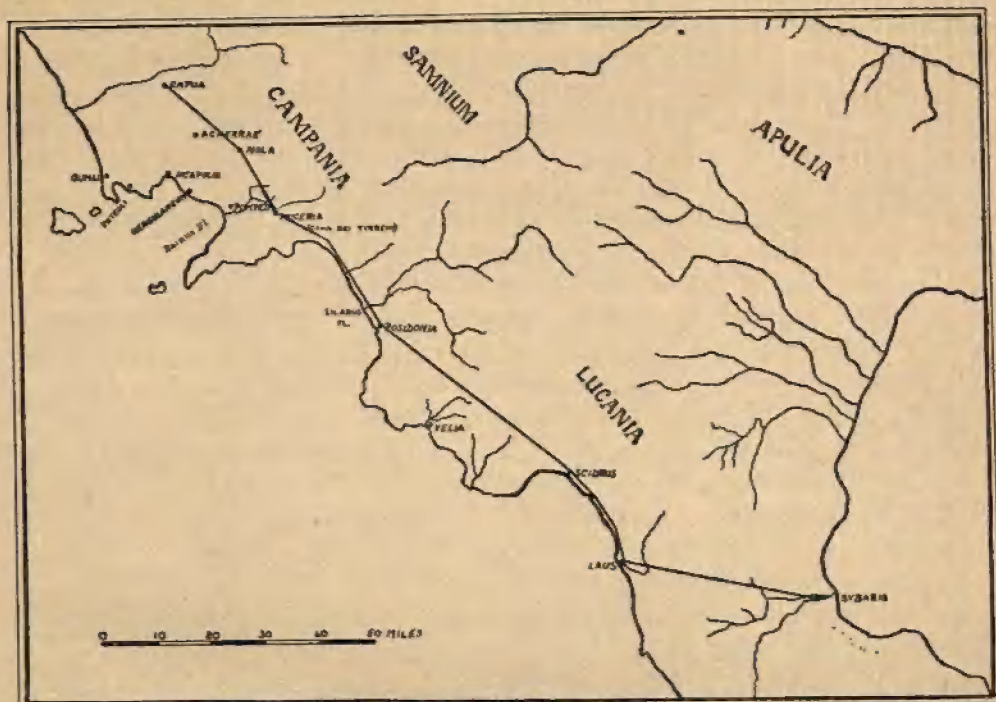


FIG. 3. Sketch-map to illustrate the trade connexions between the Etruscans and the Sybaris

too, took part in the expedition of 524 B.C. Furthermore, Nuceria occurs as the name of a town both in South Campania (fig. 3) and in the centre of Umbria; and, though its etymology is uncertain, the Campanian town is designated by a fourth century historian a Tyrrhenian (Etruscan) city.¹⁴ Not only, then, Campania as a whole, but more specifically the territory in the immediate vicinity of Pompeii

¹⁰ III, 91, 4. ¹¹ VII, 3. ¹² II, 17, 1. ¹³ *Nat. Hist.*, III, 60.

¹⁴ Philistus, apud Steph. Byz. s.v. *Νουκρία*.

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was regarded in antiquity as having at one time been occupied either by the Etruscans or by peoples who were in alliance with them. What of the Pelasgians, whom Strabo couples with the Etruscans as inhabitants of Pompeii? Elsewhere the same author preserves the tradition that the town of Caere, originally founded by Pelasgians who came from Thessaly, was subsequently captured from them by the Etruscans.¹⁵ Furthermore, in his description of Ravenna he connects the Umbrians with the Thessalians, quoting the tradition that the original founders of the city were Thessalians, who, unable to bear the wanton outrages of the Etruscans, voluntarily admitted some of the Umbrians, who continued in his time to hold the town, and themselves went back to Thessaly.¹⁶ In these two passages the Pelasgians and Umbrians are both parts of the pre-existing population conquered by the Etruscans, and both have a connexion, direct or indirect, with the Thessalians. It may therefore be suggested that the Pelasgians, whom Strabo mentions at Pompeii, are the same as the 'Umbrians and Daunians and many of the other barbarians' who are stated by him to have aided the Etruscans against Cumae in 524 B.C., and whom we have seen reason to connect with the southern part of the Campanian plain. 'Pelasgians' would, then, appear to be a general designation of all these peoples. It seems certain that, if the enlargement of Pompeii was in truth the work of the Etruscans, the actual settlers in the enlarged city would be, not pure Etruscans, but allies who had taken on the Etruscan civilization. Probably this is what is implied by Strabo's coupling the Etruscans and Pelasgians together.

At this point, however, a doubt arises. The people who enlarged the area of the town must have been the builders of the original wall,¹⁷ which, from pottery found in the earth filling, Maiuri dates to the period 520-450 B.C. Yet Maiuri is firmly convinced that the constructors of this fortification were Oscans, working under the direct influence of the Greeks. Here, however, it is necessary to distinguish between fact and theory—the date of the building of the walls and the inferences which the excavator draws from the style of their construction. The excavations supply evidence that the first wall of the enlarged city was built roughly at the end of the sixth century B.C. On the other hand, if Maiuri's view be accepted that these walls were the work of the Oscans, any theory of an Etruscan foundation of Pompeii must

¹⁵ v, p. 220.

¹⁶ v, p. 214.

¹⁷ Fig. I, A, a, A'.

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be abandoned. We must, then, examine in detail the nature of this early fortification and especially those features of it on which Maiuri bases his opinion that it was built under direct Greek influence.

The wall, as we have seen, was composed of an earth core with a double facing of limestone ashlar masonry. Only the foundations of the outer face (A) survive, the rest of it having been destroyed to make way for the Samnite wall of period II. Large stretches, however, have been found of the inner face (A') which, to the east of the Porta del Vesuvio survives to a height of 12 feet (plate 1). The courses of the masonry forming this inner facing are of varying width, and thus the work falls under Vitruvius' description of an *opus pseudisodolum*.¹⁸ The wider courses are composed of blocks which average $2\frac{1}{2}$ feet in depth and 10 inches in thickness, and which vary in length from just under $1\frac{1}{2}$ feet to just over 3 feet. At intervals along these courses, blocks are inserted endwise, so that their narrow edges only are visible in the facing, while with their length they run into the earth core, thus helping to bind core and facing together. The narrow courses in the facing serve this same purpose, being composed of blocks laid flat, which dove-tail into the earth core and bind it to the stone revetment. It is the existence in this early fortification of a double curtain, containing the earth filling, which, in Maiuri's view, is a sign of Greek construction, and which distinguishes it from the Italic system of a single revetment with a sloping *agger* inside it (as, for example, in the fortification of the Samnite period B, b, B').

This double facing of ashlar masonry and the absence of an *agger* can only be explained, he asserts, by the direct influence of Greek defensive methods such as could have been exercised by the walls of Cumae, Neapolis, or Posidonia. The whole subject is beset with difficulties, and the surviving specimens of city-walls of this period in South Italy are too few to warrant sweeping assertions. If we limit ourselves to the walls of Posidonia, which are the only ones readily accessible, we are at once confronted by the fact that they contain work of two different epochs. Maiuri has promised a detailed report of an excavation which he has recently carried out with the object of tracing the history of these walls, and such a report will be invaluable. For the moment, it is enough to notice the peculiarities of the two types of construction which the walls contain. First of all, there was erected

¹⁸ II, VIII, 17. *Isodolum*: cum omnia coria aequa crassitudine fuerint structa. *Pseudisodolum*: cum impares et inaequales ordines coriorum diriguntur.

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a fortification composed of a double facing of ashlar masonry, the courses of which are of even width (*opus isodomum*), and contain an earth filling (plate II). Then in front of the outer curtain, a new facing was built of much rougher ashlar masonry with courses of irregular width (*opus pseudisodomum*) (plates III, IV), and on the inner side an *agger* was constructed, completely covering the earlier inner revetment and thus preserving it intact. Until the report of the excavation is published, it will be impossible to date these walls with precision, but the general nature of the two stages in the fortification is clear. The original wall with the double curtain of regular ashlar masonry is Greek; the irregular masonry and the *agger* are Italic. The former, therefore, must date from a period earlier than the Lucanian invasion at the end of the fourth century B.C., and the latter from a later period. It follows that if either of these walls directly influenced the earliest of the walls at Pompeii, it was the Greek wall and not the Lucanian additions. If, however, we compare the Pompeian wall with the contemporary wall of Posidonia, we find that the differences are really more striking than the similarities.

In the first place, as we have mentioned, and as a glance at plates I and II will make clear, the blocks of which the two curtains of the Posidonian wall are composed, are long and rectangular in shape and are laid in narrow courses of even width (*opus isodomum*), while those of the Pompeian wall, though rectangular in shape, are much more square and are laid in courses of irregular width (*opus pseudisodomum*). The depths of the wide and narrow courses at Pompeii are $2\frac{1}{2}$ feet and 10 inches respectively. Those in the Greek wall at Posidonia average about 1 foot 2 inches. The former wall, therefore, is at best but a poor imitation of the latter.

Furthermore, the blocks of the Pompeian inner revetment are only half the thickness of those at Posidonia (10 inches as compared with 1 foot 10 inches); and, most significant of all, the face which they form is not vertical but, as it rises, slopes slightly towards the outer curtain, so that the fortification is narrower at the top than at the bottom. This is very important, for it means that, while in the Posidonian fortification the two strong curtains are real walls and would stand even if the earth filling were removed, at Pompeii the earth filling is of fundamental importance, since without it the inner face would not stand. For the same reason, the narrow courses occurring at Pompeii, composed of blocks running horizontally into the earth core and binding it and the facing together, and also the wedges, which

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were inserted at intervals in the wide courses to serve the same purpose, were not required at Posidonia, since the walls were thick enough to stand by themselves. In other words, at Posidonia we are dealing with two real walls and an earth core ; at Pompeii we have an earth *agger* with stone revetments. The *agger*, as Maiuri himself admits, was typical of Italic defensive schemes.

The Posidonian wall contains at intervals square towers, such as were typical of Greek fortifications, though not of Italic : in the earliest Pompeian wall no towers have been found.

Far, then, from showing signs of direct Greek influence, Pompeii's early wall is un-Greek in its use of pseudisodomic masonry, in the slope and weakness of its inner revetment, and in the absence of towers ; it is built after the manner of an Italic *agger*, merely strengthened with a facing of stone. To find a closer parallel to it we must look at the Lucanian additions at Posidonia, built in the pseudisodomic style (plates III, IV). Occurring in conjunction with the *agger*, the latter afford a proof outside Pompeii that this style of masonry was in use among Italic peoples at a period not earlier than the end of the fifth century B.C. Whence did they derive this style ? It is to be noted that the Posidonian *opus pseudisodomum* is of much poorer technique than the Pompeian—a fact which needs some special explanation, since the latter is about a century older than the former and so might naturally be expected to show a rougher and more primitive style of construction. It may be suggested that the finer technique found at Pompeii was due simply to the influence of the Etruscans, and that the survival of the use of *opus pseudisodomum* among Italic tribes a century later was a legacy from the same source.

This is in complete accord with another result of Maiuri's excavations. The present Porta di Ercolano, Porta del Vesuvio, and Porta di Stabia at Pompeii (fig. 2) are but later constructions replacing earlier gates in the original walls, and in addition a tower at the north end of the Strada di Mercurio (*i.e.*, the northern continuation of the *cardo* of the original village) covers the site of a former gate which was later disused. Hence we may conclude that at any rate in the western half of the town, the earliest street-plan coincided with the present one. Under the Porta di Nola no vestige of the early walls has been found, a fact which seems to indicate that to the east the original line of the walls was later changed. How far the first walls went in this direction can only be ascertained by further excavation between the Porta del Vesuvio and the Porta di Nola on the north side, and the Porta di Stabia and the

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Porta di Nola to the south and east. It can be shown, however, that a large portion of the original street-plan was never altered after it was first laid out, and that plan, as we have seen, was not Greek but Italic. Clearly the walls and the town-plan go together. How, then, did it come about that, if the Oscans followed the Greek model in the building of the walls, they show no sign of Greek influence in the planning of the town? Pompeii's neighbour, Herculaneum, with its parallel streets of equal importance, forming perfectly rectangular *insulae*, is as Greek in plan as it could possibly be, and clearly owed its inspiration to the Greek town of Neapolis.¹⁹ Had the Oscans of Pompeii been under Greek influence, they would not, any more than the Herculaneans, have remained unaffected by the splendid example of Greek town-planning which lay across the Bay.

The idea, then, of direct Greek influence in the building of these early walls must be abandoned. They seem, rather, to be Italic walls trying, as it were, to 'look' Greek. There remain two possibilities: either we must believe that the Oscans themselves erected the walls or we must accept the statement of Strabo and infer that they were erected by the Etruscans. A few moments' consideration, however, will show that the Oscans must be ruled out. During the sixth century, a temple was built on the 'acropolis' attached to the original village, and the orientation of this temple shows clearly that its erection antedated the enlargement from village to town (fig. 2). This temple was purely Doric in plan and ornament, and must have been put up either by Greek settlers or (more probably) by Greek workmen employed for a special piece of work. The important point is that, when the Oscans of Pompeii carried out a work under Greek influence, they came under that influence completely. There were no half measures: the temple was purely Greek. If, then, the Oscans were completely under Greek influence during the course of the sixth century, why had they ceased to be in that position by the end of the century? The only likely answer is 'by reason of the arrival of the Etruscans'. An invasion by a conquering people would give a reasonable explanation of that sudden increase in population which is indicated by the enlargement of the village into a town—an enlargement which is not explained by Maiuri's theory of an Oscan authorship for the earliest walls. May it not be that the Etruscans, or 'Etruscizing' Italians, having failed before the walls of Cumae in 524 B.C., and being impressed by their first contact with a type of

¹⁹ Beloch, *Campanien im Alterthum*, ed. 2, p. 230.

PLATE I



THE INNER REVETMENT OF THE EARLY FORTIFICATION AT POMPEII

PLATE II



A RESTORED PORTION OF THE GREEK WALL (6TH CENT. B.C.) AT POSIDONIA (PAESTUM)

PLATE III



A PORTION OF THE LUCANIAN FORTIFICATION (5TH OR 4TH CENT.) AT POSIDONIA,
BUILT AS A SCREEN COVERING THE ORIGINAL GREEK WALL

PLATE IV



ANOTHER LENGTH OF THE LUCANIAN REFACING OF THE GREEK FORTIFICATIONS AT POSIDONIA

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fortification to which they were not accustomed, tried to combine in Pompeii the merits of the Italic *agger* with those of a Greek wall, and thus produced the hybrid construction which the earliest wall seems to show? If so, this early fortification affords just the required confirmation of Strabo's assertion that the Etruscans and Pelasgians were the successors of the Oscans in the domination of the town.

A *prima facie* case has already been made out for the view that the Etruscans not only, as Strabo says, were at one time in occupation of Pompeii, but were also responsible for the enlargement of the area of the town. We will now approach the question from an entirely new angle. Assuming, for the moment, what we set out to demonstrate, that the Etruscans did in reality fulfil the rôle which we have ascribed to them, we will seek to discover a possible reason for their action and to determine when it is most likely to have taken place. We shall see that the theory of an Etruscan foundation of Pompeii enables us to explain many points connected with the coinage of South Italy, which would otherwise remain unexplained, and also that the action which we are assigning to the Etruscans fits perfectly into the scheme of knowledge which we possess of the economic situation in South Italy at the end of the sixth century B.C. It will, moreover, become clear that the colonization of Pompeii was an integral part of the wider economic policy of the Etruscans in the Bay of Naples at this time, and that, at the time of its extension, the enlarged town was designed to have an importance which it never enjoyed after the Etruscan domination came to an end. Such a study, though it begins by assuming what we are required to prove, will be found to have provided one more piece of circumstantial evidence in support of the view which this paper is upholding.

We have already referred to the passage of Dionysius of Halicarnassus in which he states that, in 524 B.C., Aristodemus, tyrant of Cumae, was successful in repelling a joint attack of the Etruscans and other Italic peoples. This shows that the Etruscans were already present in Campania. Consistently with this, Polybius states that the Etruscan conquest of the district round Capua and Nola was contemporary with their advance into the valley of the Po, which we know from archaeological evidence to have taken place towards the end of the sixth century B.C. There is reason for ascribing the beginning of the Etruscan domination in Campania to an earlier date than this, but for our present purpose it is sufficient to note that by the last quarter

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of the sixth century they seem to have been well established there. We have mentioned, too, that, though Campania was probably the southern limit of their political domination, their cultural influence spread much further south. These cultural relations were, moreover, accompanied by ties of a more material nature. A well-known passage of Athenaeus mentions the close friendship that existed between the Etruscans and the inhabitants of the Achaean colony of Sybaris; the passage implies that the basis of the friendship was commercial.²⁰ Sybaris, in fact, seems to have been the entrepôt between the Etruscans and the cities of Asia Minor, especially Miletus. These conditions imply the existence of a trade-route between the Tarentine Gulf and Central Italy, and it is important to trace the line which it followed. Its course can be deduced from a study of certain of the coin-series of South Italy (fig. 3).

Laus, situated across the toe of Italy from Sybaris, had been colonized from the latter city some time before 550 B.C., and its silver coins, most of which belong to the end of the sixth century B.C. or the first decade of the fifth, indicate close commercial connexions with the mother city. They have the incuse reverses and the cable borders of the coins of Sybaris, and the type—the figure of a bull—varies only in so far as the animal is represented with a human head.²¹ Posidonia, colonized from Sybaris in the seventh century B.C., began to strike coins in the latter half of the sixth century, not, however, on the Euboic-Attic standard in vogue among the Achaean colonies of South Italy, but on the Phocaic standard of her neighbour Velia.²² Nevertheless, relations with the mother-city are indicated by the use of a flat fabric and of incuse reverse types. At the beginning of the fifth century, a change took place, and, though a fabric different from that of Sybaris was introduced, the reverse type of the mother-city—the Bull—was adopted. This change has been interpreted as an indication of an influx of refugees from that city after its destruction in 510 B.C. It is not a far cry from Posidonia to Campania, and evidence of close commercial relations between these districts is furnished by the coinage of the Campanian cities Fenser and Hyria. Some of the types, issued by these cities in the course of the fourth century B.C., show on the obverse the head of Hera Argoia, a clear reference to the well-known temple of this goddess at the mouth of the river Silarus.²³ The type

²⁰ XII, 519 b.

²² *ibid.*, p. 80.

²¹ Head, *Historia Numorum*, 2nd ed., p. 73.

²³ *ibid.*, pp. 37-8.

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figures also on the coins of Posidonia. Fenser and Hyria can be shown to have been situated at any rate in the vicinity of Nola, and there is some reason for regarding the latter two cities as identical. These cities, then, appear to mark the line of the Etruscan communications with Sybaris, the whole route from Nola, one of the main Etruscan centres in Campania, to the Tarentine Gulf being secured by a chain of important posts—Nola, Posidonia, Laus, and Sybaris.

The southern march of the Etruscans into Campania brought them into conflict with the Greek cities on the coast, and the hostile relations, which are attested by the battle at Cumae in 524 B.C., continued till 474, when the doom of the Etruscan domination in this region was sealed by the victory of Hiero of Syracuse. Numismatic evidence suggests that this political conflict was accompanied by a gradually widening cleavage between the economic interests of the two powers.

Cumae, which at this time enjoyed a kind of hegemony in the region of the Bay of Naples, began about 490 B.C. to issue coins of the same standard as the early issues of the other Chalcidian colonies of Magna Graecia, equivalent to the Aeginetan drachm and also to one third of the Euboic tetradrachm.²⁴ Coins of this issue, however, are rare, since two new standards were adopted in quick succession. Soon after 490, a standard was followed which was certainly equivalent to the Euboic-Attic, and then, after a short period in which this and the Phocaic standard were used simultaneously, the Phocaic triumphed (about 480) and silver currency on this standard lasted until Cumae was captured by the Samnites in the last quarter of the fifth century B.C.

The coinage of Etruria presents us with two series, whose marks of value show that the unit of silver in the one case was 13.5 grains and in the other 17.56 grains.²⁵ The unit of the first series is clearly the Sicilian silver *litra*, the equivalent of a pound of bronze. The most common values (in *litrae*) of the Syracusan coins are 50, 20, 10, 5, 3, $2\frac{1}{2}$, $1\frac{1}{2}$, $1\frac{1}{4}$, and 1, and with these the values issued by the Etruscans are in close accord, *viz.*, 50, 25, 20, $12\frac{1}{2}$, 10, 7, 5, $2\frac{1}{2}$ and 1.²⁶ It is impossible, however, to correlate the latter with the form of the Euboic-Attic standard in use among the Achaean colonies of the Tarentine Gulf, for they divided the stater or didrachm into thirds, sixths, and

²⁴ *ibid.*, p. 36.

²⁵ *ibid.*, *Introd.* LIV, and pp. 14-15.

²⁶ *ibid.*, pp. 12-13 and pp. 176 ff.

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twelfths. In view of the Etruscans' known commercial relations with South Italy, these facts might seem to invalidate any attempt to use coinage as evidence of such relations. It must, however, be remembered that this series where the unit is 13.5 grains seems to derive in large part from Populonia, the port of Etruria on the west, and that no finds have yet been made which would suggest that it was also in use among the Etruscans in Latium and Campania, where they were more immediately in contact with the south. As a people, the Etruscans showed a distinct lack of capacity for national cohesion, and their tendency towards internal divisions is illustrated by the coexistence of two systems of coinage. It is quite possible that the Etruscans who conquered middle Italy merely used the coinage of the cities of the south which acted as intermediaries between them and the East. For our present purpose, the important consideration is that, when about 480 B.C. Cumae abandoned the Euboic standard in favour of the Phocaic, it was a definite sign that her economic interests were developing on lines quite different from those of the Etruscans.

Nor, again, is it possible to correlate the second series of Etruscan coins (with a unit of 17.56 grains of silver) with the contemporary coinage of Cumae, though the question which standard this series was designed to follow is still a matter of discussion. Even if Head be right in regarding it as modelled on a weakened Aeginetic standard (since five units of 17.56 grains give a total of 87.80 grains, which is roughly equivalent to the Aeginetic drachm of 90 grains), the date of its first appearance (about 450 B.C.) was later than the abandonment of the Aeginetic standard at Cumae (490) and so precludes any possibility of a connexion between the two. The economic interests of Cumae, then, in the early fifth century had drifted apart from those of the Etruscans. The adoption of the Phocaic standard is, of course, to be explained by the revival of Phocaic influence in the western Mediterranean after the battle of Alalia and the settlement of the survivors at Velia in Lucania (538 B.C.), and it must be taken as an indication that Cumae was abandoning the economic relations which she had had with the natives of the interior down to the Etruscan conquest and was seeking closer relations with such Phocaean colonies as Massilia.

With the consolidation of the Etruscans in the interior of Campania at the end of the sixth century, their foreign policy in this region was bound to develop on two main lines. It was natural, in the first place, that they should attempt to extend their zone of influence on the sea further towards the south, and, in the second place, that they should

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seek to strengthen their interests in the region of the Tarentine Gulf. One result of the first line of policy was the unsuccessful attack on Cumae in 524 B.C. In connexion with the second line of policy, a crisis arose in 510, for by the destruction of Sybaris at the hands of the Crotoniates, the Etruscans lost their chief connecting link with the East. After this disaster and in view of the friendliness of Posidonia and Laus, where the expelled Sybarites seem to have found a refuge, it was clearly to the advantage of the Etruscans to strengthen their communications with these cities in the hope of thereby being able to regain the entrepôt which they had lost. Herein lies the importance of the colonization of Pompeii. From Nola the direct route to Posidonia and the south led through the gap in the Monte Sant'Angelo in which is situated the modern Cava dei Tirreni. At the point where this pass is entered from the Campanian plain, there lay, in Roman times, the town of Nuceria (the modern Nocera), and nearby flows the river Sarno. The mouth of this river was clearly a point of strategic importance for a power which wished to hold the Nuceria gap, but whose hold on the western sea was precarious. If this point were not held, a hostile force could land without difficulty and strike a swift blow inland. The enlarged Pompeii, then, was designed to be a strongly fortified outpost, covering the line of the Etruscan communications with the cities beyond the Nuceria gap. The building of the Greek temple had shown that the enemies of the Etruscans were not blind to the possibilities of the site, but the Etruscans themselves (on the theory here presented) took no action until, with a hostile Cumae mistress of the Bay of Naples, and Sybaris destroyed, they were forced to take steps to restore their position in this quarter.

But the importance of Pompeii did not end with the protection it afforded to Etruscan communications with the south: it was integrally bound up with the first line of Etruscan policy which we have distinguished, *viz.*, the extension of their zone of influence on the sea. Strabo speaks of Pompeii as the port of Nola, Nuceria, and Acherrae,²⁷ and, considering the distance of the country behind Vesuvius from Puteoli, the leading port of the Bay of Naples in Strabo's time, it is comprehensible that a subsidiary trading-post was required in the south-east. It is indeed strange that Acherrae, a town situated eight miles to the northeast of Naples, should have used a sea-port so far to the south. But, however this is to be explained, Strabo's remark is

²⁷ v, p. 247.

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significant as showing how wide were Pompeii's inland connexions in the Augustan age, when Campania and the Bay of Naples were no longer divided between conflicting powers, and there was no political reason for using one sea-port in preference to another. At the beginning of the fifth century B.C., Naples would undoubtedly have been the most convenient outlet for the Etruscan centres at Capua and Nola. This way, however, was already closed, for the site of Naples had been occupied on two distinct occasions by settlers from Cumae, and its coinage, which begins about the middle of the fifth century, shows that by that date it had not shaken off the domination of the mother-city.²⁸ A strong power in the interior of Campania could not allow the means of egress both to the north and to the south of Vesuvius to be closed. Too late to occupy the exit to the north, the Etruscans seized that to the south, and Pompeii thus served the double purpose of a sea-port for the cities of Campania and a guard of their communications with the south.

In order that Pompeii might serve the purpose which we seek to assign to the action of the Etruscans, it was imperative that it should be kept in subordination to the towns of the interior. There is no direct evidence of such dependence during the Etruscan period, but indirect evidence is furnished by the entire absence of any sign that Pompeii ever had a coinage of its own, a strange phenomenon in view of its size and of the number of towns which are known to have issued coins in the early Samnite period. Nuceria, from the beginning of the third century B.C. till the Social War of the Sullan age, seems to have been the head of a league of cities in South Campania, of which Pompeii was one, and it is quite possible that this dependence on the interior was a legacy from Etruscan days.

We have, then, considered evidence on four main points : (1) the plan of Pompeii and the shapes of its house-blocks are Italic ; (2) there was a strong tradition in antiquity that the Etruscans and their allies were at one time in possession of the southern part of the Campanian plain : moreover, Strabo affirms that the Etruscans and Pelasgians (*i.e.*, peoples conquered by the Etruscans or allied with them) succeeded the Oscans in the occupation of Pompeii ; (3) the first city-wall of Pompeii, constructed precisely at the time when the Etruscans were present in Campania is not Greek but Italic : it shows, however, a

²⁸ Beloch, *op. cit.*, p. 29.

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higher technical skill than the Italic tribes of the immediate neighbourhood possessed, and therefore seems to have been constructed under the influence of some highly civilized power in the interior of Italy; (4) the economic and military position of the Etruscans in South Italy, in the years which immediately followed the destruction of Sybaris in 510 B.C., was such as to give them strong reasons for desiring to establish a well-fortified colony at the mouth of the river Sarno where Pompeii is situated. The results of our investigations on these four points are absolutely coherent. They point to the conclusion that the Etruscans colonized Pompeii about 500 B.C. to guard their communications with South Italy and to obtain a *point d'appui* against their rival Cumae. If this conclusion is correct, the action of the Etruscans represents a not unimportant incident in the general conflict which was being waged, at the end of the sixth century B.C., and the beginning of the fifth, between the Etruscans and Carthaginians on the one hand, and the Cumaeans, Phocaeans, and Syracusans on the other—a conflict which, in its turn, formed part of a wider struggle going on throughout the whole Mediterranean at this time, between the swiftly developing civilization of Hellas and the older civilizations hemming in the Mediterranean (Oriental, Carthaginian, and Etruscan).

Cross-Dykes

by J. P. WILLIAMS-FREEMAN

SHORT cross-dykes are found on ridges or more rarely in valley bottoms. Their essential characteristics are that they run across a narrow strip of open ground, their ends resting on obstacles which in primitive times would have been naturally impassable :— in the case of cross-ridge dykes from scarp to scarp or scarp to forest, and of cross-valley dykes across the hard gravelly bottoms between impenetrable woods. In every case that I have seen they cross the line of a primitive road.

They may be 'univallate', *i.e.*, consist of a single bank and ditch, or 'bivallate', *i.e.*, having a single ditch sunk between two banks; and they may be of any size: 'large', *i.e.*, according to the Earthworks Committee's classification of over 10 feet crest-ditch vertical height, 'medium' between 10 and 4 feet, or 'small' below the last figure.

They occur on most ranges of hills in England; they are found on the Berkshire Ridge, and are very common on the East Yorkshire wolds, but it is on the Chalk ridges of Wilts, Dorset, Hants and Sussex that they seem most numerous and have attracted most attention. Many doubtless exist which have not been recorded, but in the three first-named counties alone three have been noted across the East Wilts 'Oxdrove' along the West Hants border, and Mr Heywood Sumner¹ has described in South Wilts and Dorset no less than eleven across Whitesheet Hill (the old Shaftesbury-Sarum road), and nine across the Oxdrove ridge of Cranborne Chase. But it is on the South Downs between the Itchen valley in Hampshire and Willingdon Hill in East Sussex that the most striking series occurs. Here there are at least 39 and probably more. Most of these are to be found west of the river Arun where the southern slope of the chalk range is wider, gentler and more wooded, and all the complex groups are found in this section. It is this West Sussex series that Dr Eliot Curwen and his son,

¹ *Earthworks of Cranborne Chase*, p. 62 et seq.

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Dr E. Cecil Curwen, specially investigated and described in their well-known paper² which must form the basis of all serious study of these earthworks.

The 'large' class cross-dykes are very few—not much more than half a dozen (including four in one group) in the four counties: each seems to have been constructed for a special defensive purpose and must be studied separately. It is the 'medium' and 'small' class, mostly with a crest-ditch vertical height (CD, vert¹) of under 6 feet, that seem to fall into a class by themselves and can be studied together.

Their essential characteristics are well marked: their ends rest on the steepest slopes often at the heads of coombes, or upon large or small patches of thick impenetrable wood, and they all cross an old track, often the main ridgeway where one or two spurs with their secondary ridgeways have converged upon it. The track may pass through a simple gap—in no case defended—which may or may not be the original way through, or it may pass the end of the cross-dyke; in some cases most uncomfortably near the steep edge of the scarp. There can be no doubt that the position of nearly all cross-dykes is eminently suitable for obstruction of the road.

Apart however from this general resemblance cross-dykes and their characteristics show considerable variety: they can be discussed under the following heads.

- (1) Single univallate entrenchments.
- (2) Single bivallate entrenchments.
- (3) Double, treble or multiple contiguous banks and ditches.
- (4) Groups of the above elements, either of similar or different types.

1. THE SINGLE UNIVALLATE ENTRENCHMENT. This 'defensive' type (though for these little earthworks 'obstructive' or 'protective' would be a better term) is of course the simplest form of ancient entrenchment—a bank thrown up from a ditch on the enemy side—such banks and ditches are an extremely common form of cross-dyke. They may sometimes be very small—only 1 or 2 feet 'CD, vert'—that is, not bigger than a hedge bank, and so often escape notice; but it is to be observed that even the smallest often choose a defensive

² 'Covered Ways on the Sussex Downs', *Sussex Arch. Collections* 1918, LIX, 35-75.

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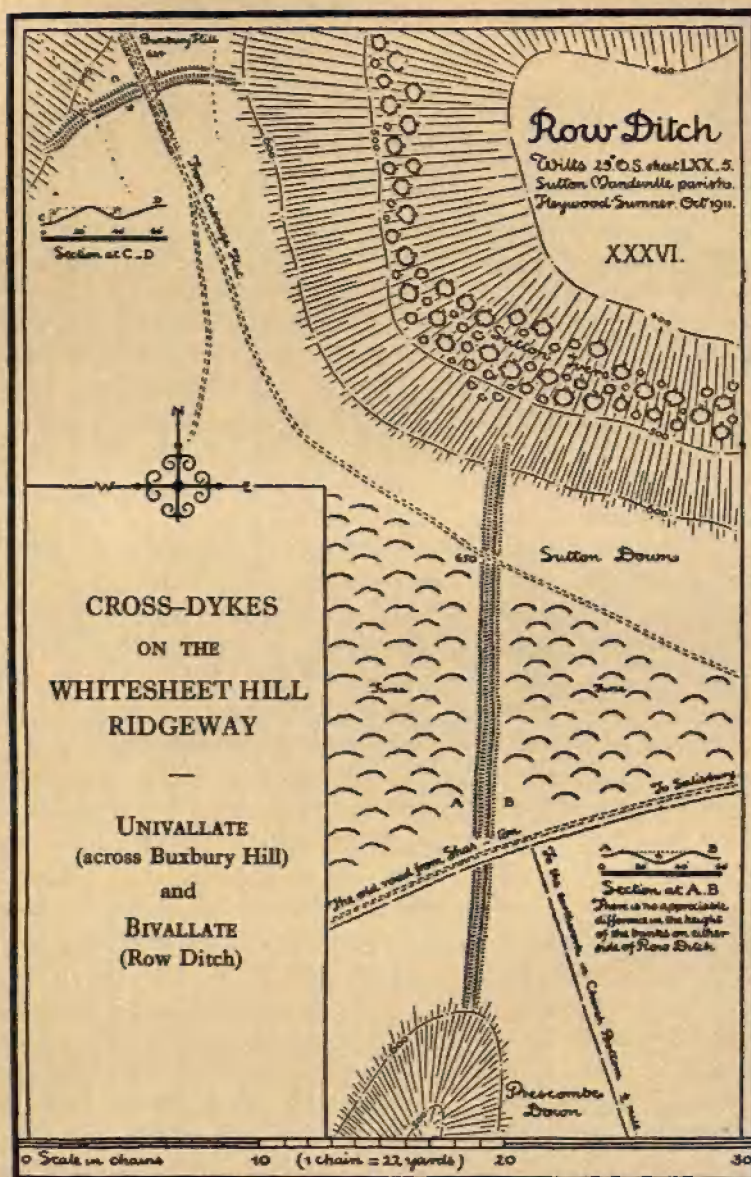


FIG. 1. By permission from Heywood Sumner's *Earthworks of Cranborne Chase*

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position. They may well have been, like hedge-banks, merely protective to feeding grounds or cultivation, while the larger ones may have been designed to hold up travellers and cattle going along the ridge and demand toll for free passage.

The rule that for defence the ditch must always be on the enemy side does not I think always hold good when the ascent up to the entrenchment is very steep: there are instances where the earthwork from its position has every appearance of having been constructed for defence, but where the makers have been content to take, as it were, the steep slope of the hill for their ditch and construct the entrenchment in the easiest way by throwing up the bank from above. Small banks with ditches on the wrong side may however be modern work—sham fights innumerable have been fought on the South Downs and volunteers' shelter trenches have to be borne in mind.

In one of the best examples of a single cross-dyke of defensive type (West Harting Down) the entrenchment terminates in a sort of rough shallow pit on the edge of each scarp. These may possibly mark the position of some sort of block-house to protect the ends.³ Caesar, describing a short entrenchment he made in Gaul, which appears to have been a cross-dyke, states that he put 'castella' at the extremities for his military engines.⁴

2. SINGLE BIVALLATE DITCHES. These are by far the commonest form of cross-ridge dykes upon the chalk and differ essentially from the univallates in that it is the ditch that is the important element; in worn or ploughed down specimens it is this member that can often be followed where the banks have practically disappeared. They consist essentially of a ditch sunk between two banks—banks which typically are of equal size. In many cases, however, especially along slopes, the banks are now unequal and sometimes, especially in the larger examples, one bank so predominates that it is difficult to distinguish the entrenchment from one of the defensive type with a bank on the counterscarp.

In size these cross-dykes have usually a 'cd, vert' of 4 or 5 feet with an overall horizontal measurement of 40 or 50; thus exactly

³ There is just such a shallow pit at the southern end of Grim's Bank on Little Heath, northwest of Silchester, 70 yards north of the Roman road leading to Speen.—EDITOR.

⁴ Caesar, B.G. II, viii.

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corresponding in profile to the long cross-country 'inter-settlement' ditches which are so common on Salisbury Plain and the open chalk generally—entrenchments which Colt Hoare christened 'Covered Ways', and are often described as roads or boundary ditches, but which there is a growing tendency to look upon as ancient cattleways. Sections cut by the Curwens⁵ agree very closely with those of Old Ditch on the Plain and of other such ditches. There is the same average depth of the ditch of about $4\frac{1}{2}$ feet below the natural surface of the ground, the same flat narrow bottom of 1 to 2 feet, and the same trumpet-shaped section, steepening rapidly to the bottom, which we know from Dr Cecil Curwen's recent investigation to result from the weathering of a nearly vertical-sided ditch.⁶ The resemblance in construction between these cross-country and cross-ridge ditches is so close as to amount to identity.

The ends of the bivallate cross-ridge ditches are often prolonged, slanting down the sides of the Down as sunken or beaten paths, and occasionally as definite earthworks; and they may coincide with property or parish boundaries. This again corresponds with the ditches on the Plain where a track often continues their line for long distances.

Another occasional and very curious feature of this type of cross-dyke is the 'kink'. The ditch will take a sudden nearly right-angled turn for 30 or 40 feet and then turn again to continue its original alignment, the foremost bank taking the line of the rearmost. In every case an old track goes through the entrenchment at the 'kink', usually, if not always, flanked by a low turn of the bank. There are three such 'kinks' in bivallate cross-dykes in Sussex and another (as also one in Dorset) where the line of the banks suggests it, but an unusually broad road through has destroyed them too much to be certain. In the East Yorkshire dykes several such 'kinks' are figured by Mortimer⁷ as well as other entrances and corners of a more complicated character.

3. DOUBLE AND MULTIPLE BIVALLATES. There are three pure examples of these cross-dykes known to me: on Twyford Down, near Winchester (double) and at Leydean near East Meon (treble) on the South Downs, and at Thickthorn Down in Dorset, also treble. Two other similar triple entrenchments exist—on Launceston Down

⁵ *Covered Ways on the Sussex Downs*, pp. 63 et seq.

⁶ *ANTIQUITY*, March 1930, p. 97.

⁷ J. R. Mortimer, *Forty years Researches*, 1905.

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and at the great British village on Gussage Down in Dorset—but they are not cross-dykes. They lie on comparatively level Down—in indeed in the three first examples, though they are definitely across ridges and ridgeways, the ridges are not very narrow or sharp.* They are beautiful, regular little earthworks from 300 to 500 yards long; a band of even contiguous banks and ditches, with a general height of from 4 to 6 feet,

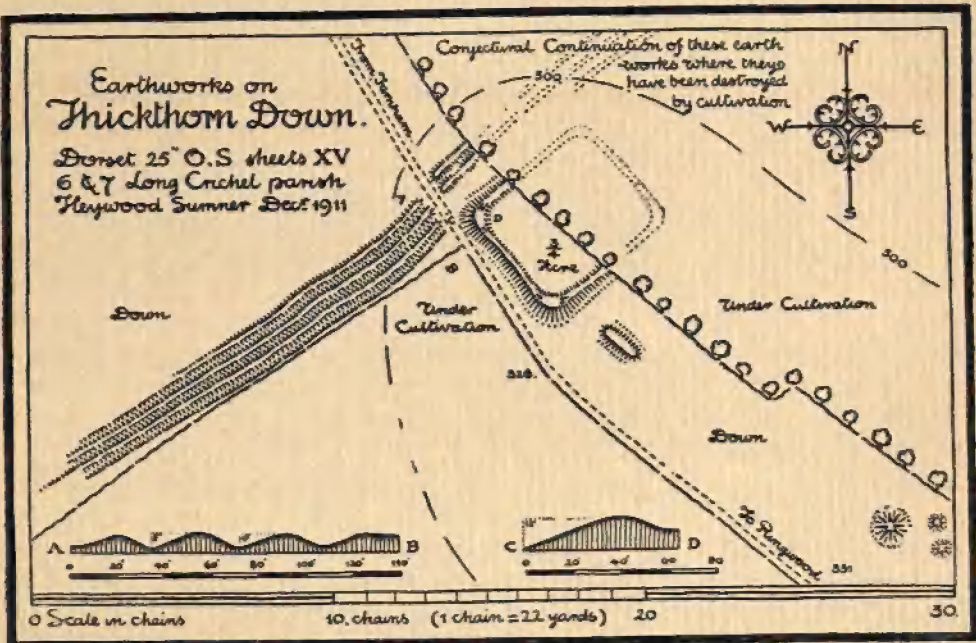


FIG. 2. MULTIPLE BIVALLATE CROSS-RIDGE DYKE
By permission from Heywood Sumner's *Earthworks of Cranborne Chase*

and in the case of the triple ones an overall horizontal width of about 120 feet. Though the southern chalk cannot boast of a set of more than three ditches between four banks, Mortimer describes them as common up to six ditches and seven banks, and even figures one of no less than seventeen ditches and eighteen banks. Most of these are across ridges or high ground. Mortimer specially notes that his multiple ditches often diverge at their ends: both the South Down examples have tracks leading from their ends, and at Leydean at one end the ditches diverge—the other has been destroyed.

*Two cross-valley dykes of this type have recently been noted in Hampshire.

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From Dr Cyril Fox's description of the Mile ditches near Royston^a and mention of two others in Bedfordshire and Norfolk it would seem that this class of short entrenchment may be much more widely distributed.

Besides these pure examples of multiple bivallates, evidently constructed together as one system, in no less than five of the groups to be described next there occur contiguous banks and ditches of a much less regular and more haphazard composition, which though they may have served the same purpose by no means conform to the same type.

4. GROUPS OF CROSS-DYKES. (A) Uniform. There are in Dorset and Sussex three or four groups consisting only of two or three small univallate entrenchments a few yards apart, one behind the other. They are placed across a main ridge or more often across spurs as if for protection. (B) Mixed. The more important form of group however which Dr Curwen first described and planned is of a much more mixed and complicated character, so much so that it is sometimes difficult to analyze them into their component elements. There are five such groups on the South Downs, all as has been said on that part of the range where its long southern slope bears a scrub of varying thickness (now much replaced by beech plantations) with patches of thick woods especially in the valleys.

Each one of these groups is made up of both kinds of earthwork, each is placed in a strong defensive position as regards the ends of the cross-dykes, and in four out of the five the beginning or end of the group (and in one case both) is protected by an univallate entrenchment facing outwards along the ridge; the fifth is doubtful. The banks and ditches may be in sets, sometimes separated by 30 or 40 yards or more; the component members of the set may be of both kinds, they may be actually touching or separated by a few yards—the arrangement seems to be haphazard but the bivallates always predominate and the impression given is that the ditches were the important part of the system. The total depth of the group along the ridge may be as much as 370 yards, and the number of component cross-dykes may vary from three to seven or eight. There is again a marked tendency for the ends of the cross-dykes to diverge from one another and to be continued as tracks along the slopes of the ridge.

^a *Archaeology of the Cambridge Region*, p. 127.

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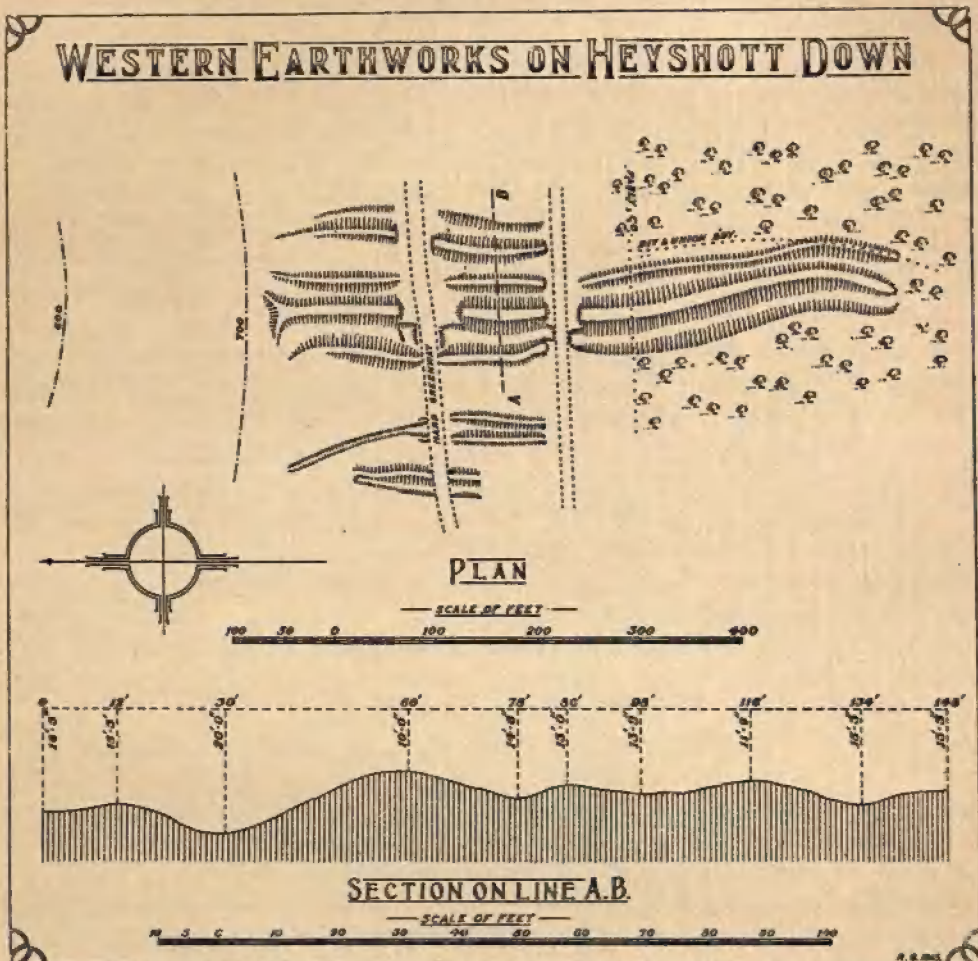


FIG. 3. GROUP OF MIXED CROSS-RIDGE DYKES
 From E. and E. C. Curwen, 'Covered ways on the Sussex Downs'
 By permission of the Sussex Archaeological Society

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As regards the age of construction of cross-dykes the only clear evidence so far concerns the typical single bivallate example on Glatting Down. That is undoubtedly older than Stane Street, which passes over it, but as to how much older the evidence of a section cut by Dr Curwen⁹ was somewhat inconclusive, though he puts it in the late Bronze Age. In one or two cases on the South Downs a barrow interferes with a cross-dyke and appears to be later, and in another (a multiple bivallate) it appears to be earlier, but the superficial evidence is not clear, and in any case the age of the barrows is unknown.

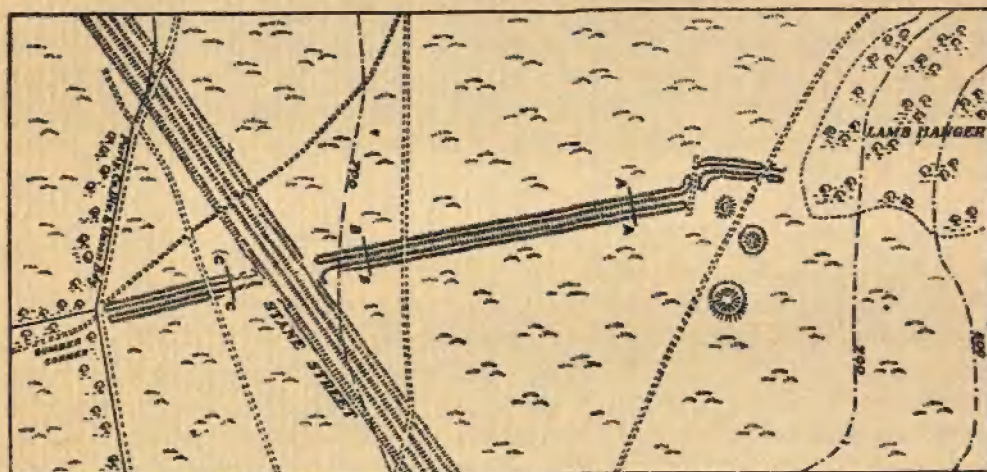


FIG. 4. GLATTING DOWN, SUSSEX
Bivallate cross-ridge dyke, showing 'kink' and Stane Street crossing dyke

— SCALE OF FEET —
100 0 1 2 3 4 5 6 7 8 9 10 11 12 1300

From E. and E. C. Curwen, 'Covered ways on the Sussex Downs'
By permission of the Sussex Archaeological Society

The evidence that the East Yorkshire multiple dykes are later than the Bronze Age barrows appears to be conclusive; and the long bivallate ditches on Salisbury Plain have in several instances been proved to be not earlier than the Iron Age.

The case for connecting this type of linear earthwork with cattle is a strong one. So much attention has been directed of late years to the agricultural field-system of Celtic times, that study of the pastoral

⁹ *Covered Ways*, pp. 62 et seq.

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conditions has perhaps been somewhat neglected. But the more we know of the high development of cultivation and agricultural settlements the more we must recognize that the need for cattle management must have advanced with it. The free roving conditions of pastoral nomad life were no longer tolerable: paths to which cattle could be confined for the protection of the crops, enclosures and regular stations for the cattle industry and trade must have been ultimately needed. For the first purpose the cross-country bivallates seem well adapted: their narrow floor 4 feet below the surface, their confining banks, their radiation from settlements, their leading to ponds, their skirting cultivation areas, their turns and twists at field corners all fit in. But can the bivallate cross-ridge dykes be brought into the picture? Their position between the steepest scarps, where there are far greater stretches of Down with gentler slopes, and numerous low dips and saddles which might equally well have been chosen, puts it to my mind out of the question that they should have been constructed as roads or covered-ways across the ridges, either for cattle or for men: nor has any explanation been offered for several such covered-ways existing side by side.

I suggest that these short ditches were constructed not for cattle-ways but for cattle-pens—the single ones perhaps for confining the travelling or other cattle at night, and the compound ones for collecting, sorting, and marking the cattle of the community. If such were the case most of their peculiarities—the diverging ends, the paths up the slopes, the ‘kinks’ and the covering protective banks—would be readily explained; while their position and the presence of anomalous defensive banks amongst them can be explained as being due to the universal tendency for peaceful establishments to grow out of others on sites originally chosen for their strength.

The question is, are they suited for such a purpose? Unusual though such a construction may seem to us, I think they are. Consider their original section. A ditch, 1 or 2 feet broad at the bottom, sunk 4 feet deep in the ground, with steep sides and with banks rising some 6 to 10 feet or more above the floor, with or without a fence at the top, would be a pretty safe place to stand even the active little cattle of the day. They would have small chance of turning round or stampeding if the ends were closed by some sort of gate or fence, and would be under the control of a man standing on the bank. To a pit-dweller used to working in earth such a construction would not be unnatural, and one has only to look at the earthworks in Rewell Woods near

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Arundel, discovered and planned by the Curwens,¹⁰ which I believe to have been a cattle compound, to realize the extent to which digging has been carried. Here there are over 3 miles of ditches remaining, included in an area of over 80 acres, all of which appear to have been bivallate, except a piece of the enclosing entrenchment.

Apart from its use as a highway, the open ridge of the South Downs must have been of great importance in pastoral times. Where these compound systems of bivallate ditches occur, the woods and scrub come not only to its foot on the north, but clothe the long slope of 4 or 5 miles stretching down to the south before reaching the flat tertiary forest-covered plain of the coastal districts. Beechwoods now cover much of the slopes, and thicker woods fill the valleys. The bare chalk spurs such as Bow Hill, Fernbeds Down and Bexley Bushes have their slight earthwork enclosures, doubtless pastoral settlements such as we find in Wilts and Dorset ; and the main ridge has earthworks along its line. The half-wild cattle belonging to each community must have wandered free over the unenclosed slopes and, as in all pastoral countries at the present day, there must have been a great rounding up at least once a year for the sorting and marking of the young stock, gathering in the milch cows for the support of the settlement, the supply of trade and other purposes. One can picture the driving in of the cattle along the well-worn tracks converging on the open hill-tops, where alone they could be mustered and dealt with, much as cattle are brought in today in the wide Estancias of South America, the banked up or wattled 'slunway' taking the place of wire. The end of the journey at an Argentine station is made in a 'cattle-race' consisting of sloping wooden walls with a very narrow bottom along which the cattle are driven in single file without a chance of turning round. Across the end of such a race is a bridge on which a man stands and operates a swing gate, sorting the beasts as they come towards him and turning them right or left into their appointed paths.

We have on these ridges ditches capable of confining cattle, pathways sloping up to them, 'kinks' which suggest gateways, and covering banks which suggest enclosure. We have a right to ascribe to their makers skill in the construction of wattle fences, gates, swing bars and woodwork of all kinds. We have only to supply a little ingenuity and imagination to reconstruct the whole scene.

¹⁰ *Sussex Archaeological Collections* 1920, vol. LXI, 20-30. [These will appear on the forthcoming revised edition of the 6-inch Ordnance Map.—EDITOR].

A Pedigree of Anglian Crosses

by W. G. COLLINGWOOD

SCULPTURED stone crosses are simply a local and temporary fashion in gravestones. They came into vogue when the northern English church-builders learned stone-carving, and their use spread in every direction through the British Isles, where it lingered on the Celtic fringe after the fashion had gone out in its first home, and afterwards it wandered farther afield. But by Anglian crosses is meant here only those set up under the influence of the Angles of Northern England before the Danish invasion, and those in which the same influence survived in parts not dominated by Danish or Norse settlement.

Before the invention of the Anglian cross, carved with ornament and showing a cross-head standing free against the sky, there were pillars and obelisks marked with the sign of the chrism or the cross. These are not properly crosses, as the word is used here, though out of them, no doubt, the Anglian type developed. The date of this invention of the free standing stone cross is not yet generally accepted : the older antiquaries held that it was in the 7th century and well before the time of Bede. The main reason for this belief was that on the best-known of the series, that of Bewcastle, the names inscribed are of persons who lived about 670-680 : and the dating of all the rest has been fixed mainly by the attribution of this one cross to that period and by little else in the way of argument. In this paper it is proposed to shift this date of the beginning of Northumbrian crosses about 60 years later, *i.e.*, 740, instead of 680 or so, which has been the popular date for the start of Anglian monumental sculpture.

It seems a trifling point, but that judgment may not go in default, and since explanation is needed, I should like to set down, in as few words as possible, what I conceive to be the development and chronology of our early Christian monuments. For figures I refer to my *Northumbrian Crosses* or any other illustrations, hoping that the reader is not unacquainted with the principal examples.

Though the later works of this movement are multitudinous, those strictly to be referred to the period before the Danish invasion

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are not so very many. Of the pure Anglian schools (740-900) there are not more than about 50 pieces known, reckoning as units those which can be restored by putting together existing fragments. No doubt this does not represent the whole output of the age, but these monuments were costly and presumably therefore rare. Their remains have been so long known that further description is hardly necessary in most cases.

This tends to indicate that we now have seen the greater part of the original series, enough to reason upon. Of late, new finds have represented chiefly the age following after the Anglian: most of the new stones are in a fragmentary condition and have been unnoticed for that reason. Some few of the class may still turn up, but if we can place what are known into a series we have solved the problem provisionally, and more cannot be expected of archaeology, unhelped by written history. But to do less is unscientific, and we must take them as a class interrelated, not separately originated—each a *lusus naturae*, as the ancients regarded fossils.

It is unlikely that our crosses were independent of parentage and made by different workmen, mostly foreign, the artists or groups of men creating, in every case, a new design; because that would imply their importation or imitation from a foreign model, which does not exist. If the Reculver fragment is Saxon and carved about 670 (Clapham, *English Romanesque Architecture*, p. 62 and pl. 20), its unique character is such that we cannot bring it into our series. It would be simply a 'boulder in the formation', an intrusion without parents and without progeny. Its lines and relief are far more subtle and accomplished than anything else in Anglo-Saxon art of anywhere near that time. It cannot be called a cross among those we know; if it was such a work it is extraordinary that it should have left no impression on the art of the 7th century or for 100 years later.

All the crosses before the time of Bede—and there were no doubt many—seem to have been of the kind set up by Oswald at Heavenfield or by the Irish at Iona, that is to say of wood; or else simple columns of stone, unornamented and rude. The reason for this generalization is that we have no convincing allusion to sculptured stone as monumental art in that period. Those mentioned by William of Malmesbury and others are dated by modern writers only from the personal names attached to them. Several known examples, similarly attributed, can be explained as belated memorials of persons long dead, but for some reason commemorated by the piety of after ages. Such must have

A PEDIGREE OF ANGLIAN CROSSES

been the cross to Paulinus at Dewsbury, on which Leland saw the saint's name ; for long after his flight from Yorkshire in 631 Dewsbury was a place where no such monument could possibly have been erected. Such also was the Hackness cross to the unknown Ethelburga, obviously a belated memorial. Such, I think, was the Bewcastle cross, perhaps a hundred years later than the persons it appears to name ; I have suggested an explanation of this in *Northumbrian Crosses*, p. 116 ff. And the two great monuments at Glastonbury described by William of Malmesbury (I, 20)—one 26 feet high in four storeys, naming men who died at various dates before 705, and the other 28 feet high with five storeys—need not be taken as carved at the date of the persons recorded upon them. These monuments are also mentioned by Wendover as near 'Arthur's' supposed tomb. I don't doubt their existence, nor that of Arthur's tomb, whatever it was, but only their date, as contemporary with the names read on them. They are no evidence of great stone crosses before Bede's time.

A curious example of a work of art standing out of the Anglian series is the slab which can be restored from pieces at Hexham church and in Durham cathedral library, representing a child as archer with animals and birds among tendrils of vine. It is said by some that it cannot be Roman because there is nothing to match it in the region of the Wall. Rivoira classed it as Anglian, and others follow him. Its exact find-spot is unknown, but it certainly is quite unlike any Anglian work in the carving and design. It is more like the stone with birds and wreath (figured *Northumbrian Crosses*, p. 22) at Hexham church, which is known to have come from the crypt, and therefore cannot possibly be part of an Anglian cross ; it was built in at that place as valueless by Wilfrid's men, like the well-known Roman horseman discovered in the foundations. This must have been about 678, implying that up to then decorative carving from Corstopitum or other Roman sites was regarded as rubbish, neither material nor model of ornament, though used in Wilfrid's building merely to fill up gaps, not to be seen.

ACCA CROSS (fig. 1). But Acca (bishop of Hexham 709-732 ; d. 740) brought foreign art to Hexham and both enlarged and embellished Wilfrid's church (*Bede's History*, v, 20). His additions, I think, can be distinguished from Wilfrid's rudimentary work of a generation earlier (see *Arch. Aeliana*, series 4, vol. I, 65 ff.). At Hexham he must have had a body of workmen able to do stonecarving—at any rate

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they were living there while he was bishop, and when he died after eight years' exile some skilled foreign artists, probably Italian—we have no trace of Orientals—may have set up the cross now at Durham to his memory at Hexham. This is thought to be Acca's monument because the parts found at and near his grave tally with

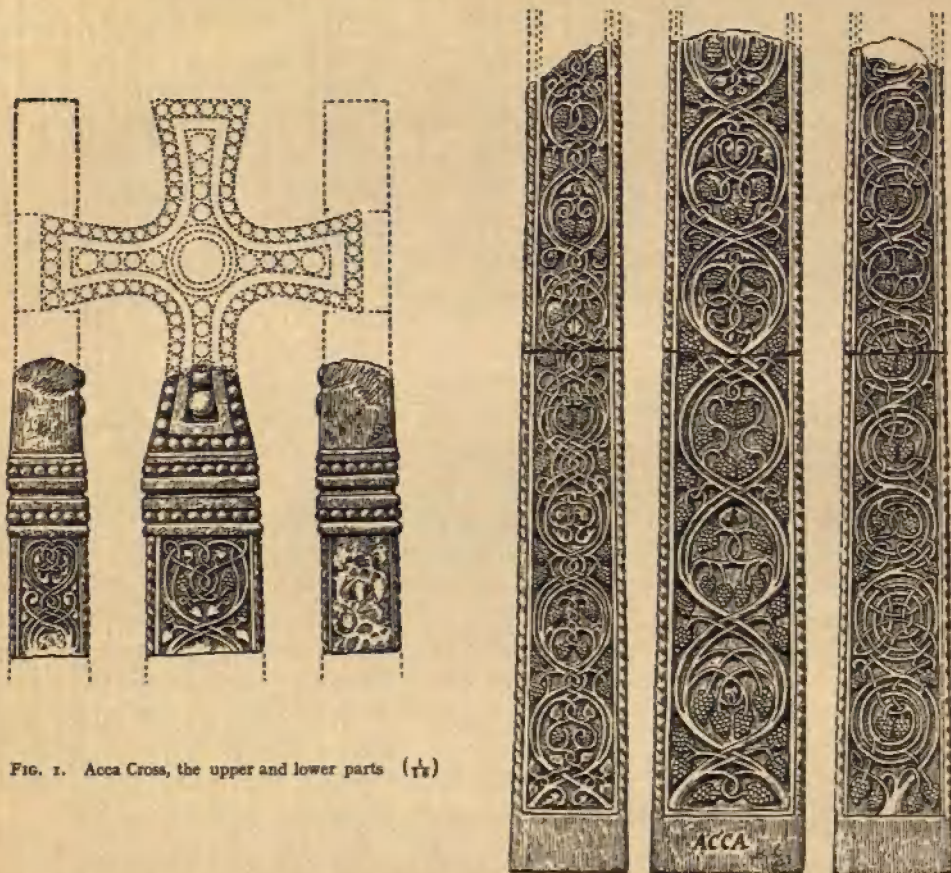


FIG. 1. Acca Cross, the upper and lower parts (1/11)

Symeon of Durham's description of the place as it was in the 12th century (*Hist. Regum*, s.a. 740). The name of Acca does not appear on this stone as we have it and there may be some doubt. But even if this is not Acca's, nor of his date, there is that about it which makes it the parent of Anglian crosses.

It does not stand apart from the Anglian crosses, like the Reculver stone or the Hexham archer, for it is obviously of the Hexham series.

A PEDIGREE OF ANGLIAN CROSSES

It is the one thing which is thoroughly un-English and yet cannot be assigned to any definite foreign origin. On the one hand this Acca cross is not native English : no Englishman at any time in the Anglo-Saxon age conceived such a set of artistic and delicate lines. There is nothing in our ancestral art to match it for variety and beauty as a set of variations on a single theme. All of the 24 remaining loops (some being lost) are perfectly harmonious and carry out the same motive without the introduction of any contrast but what is in keeping with the fundamental pattern. It is not on the other hand a foreign import or imitation. Monuments like the Roman pillar of Igel (near Trier ; 21 metres high and carved on four flat sides, *c.* 200 A.D.) cannot have given suggestions, and the cross-head is wanting in Asiatic gravestones. No Oriental or West European monument shows such a head, very simply planned and suggesting that this addition to an elaborate shaft was a novelty and an experiment, designed by an innovator. This head betrays a wooden construction, with its superimposed cross edged by mere pellets, imitating nail-heads. The cross is not in any way influenced by any other known cross. This imitation of woodwork and the naturalism of its ornament show that it is early : a later designer would have fallen into one of the conventions already established. It has no real vine-leaves, showing that this motive, tempting to a southern artist, was not present in the mind of its creator. It has the Roman form of scroll, with junctions everywhere interwoven, not like the Syrian links of stiff lines. One loop is formed of a leafless knot of bare boughs, but this is not treated like later interlacing, as a patch of different character on the design, but in harmony with the rest so that it almost escapes notice. It is true that the detail is now worn in places, and difficult to follow : more than one interpretation of some parts is possible ; but the character of these forms cannot be mistaken. It is the unique design of a man working at Hexham before the Anglian crosses had received any conventional type which he had to follow.

THE HEXHAM SCHOOL

From this Acca cross as an unapproachable model, and in more or less clumsy 'English' imitation of it, one can trace a definite style through examples of the Hexham and Lancaster schools. This is the 'pedigree' of the earlier Anglian crosses—their connexion, linked in series, to which I now call attention. Why it happened to be transplanted to Lancaster it would be idle to ask, but that such was the fact the remains prove. I suppose workmen from Hexham, for some

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reason which we do not know, were invited to Lancaster abbey, of which we have traces in its monuments, but no literary evidence, and it must have existed as an important centre before the Viking age.

2. The Stamfordham shaft, perhaps originally at Hexham or at any rate by Hexham carvers, is the natural outcome of imitating Acca's cross. Joints were thickened, for which there was full authority in Roman work, and the whole is much simplified, but nothing is original or not derived from the model.

3. The great cross (Durham cathedral library iv) originally at the foot of Acca's grave, though not necessarily connected with that burial but merely its accidental neighbour, might almost be the work of the carver of the Stamfordham fragment. The differences are only in small detail of toothed leaves. But it must be noted that the lowest loop on the broad side, resembling the Acca cross in being a knot with no leaves or berries, shows its source. The work must be an imitation of Acca's cross.

4. Lancaster I is in the Hexham style, even in having a bare-boughed tangle, like the last ; but its joints do not thicken, and on the narrow side the boughs run in more flowing curves, not so tightly drawn.

5. At Kendal church is a small piece of a cross either from Lancaster or of Lancaster style, which means a distant origin in Hexham.

6. The Spital shaft is like Stamfordham, but one side has the figures of the Crucifixion added ; indeed it might almost be part of the Stamfordham shaft if it fitted the dimensions. This need not be much later than the date of Acca's death. An English carver set to imitate Acca's cross would produce something of this kind. No length of time is required for its 'evolution'.

The following are later but in the same tradition.

7. The 'Hardwini' cross at Lancaster is another of pure Hexham origin, though somewhat late.

8. At Heysham near Lancaster the 'doll's house' cross has scrolls of Hexham form, but its knotwork shows it to be of the 9th century, although of the same tradition.

9. The great cross at Heversham (fragmentary), in which beasts are introduced, is evidently of the Lancaster-Hexham school, but with new motives added.

A PEDIGREE OF ANGLIAN CROSSES

10. The cross which I restore (*Northumbrian Crosses*, fig. 42) from Mr Lockhart's piece of a head, the shaft in Durham cathedral library (no. v) and the base in Hexham church, is plainly in the tradition of Hexham: its period is shown by its triquetra and the elaborate base with late knotwork which fits it.

11. The cross I put together from pieces at Simonburn has a Hexham scroll with later developments in the way of birds and flowers, but still of Hexham origin.

12, 13. And at Nunnykirk and Falstone are fragments of late development, much varied from the original source, which derives ultimately from the same motives, in the scrolls of Hexham.

These last six are of various dates in the age following Acca's death, but by workmen deriving inspiration from Hexham conditions.

HEXHAM'S RIVAL : THE SECONDARY SCHOOL OF NORTHUMBRIA

At some early date a rival style sprang up which perhaps may be attributed to the workmen of Benedict Biscop's foundations in county Durham, though the chief reason for the guess is that Benedict's foundations were the known rivals to Hexham. This style improved on the simple scroll by adding birds and beasts, and plaits contrasting with them—in fact, using in stone the stock manuscript illumination patterns. It may well be asked, why should these not be dated earlier than the Hexham style? The answer is that if it were so, why the pure Hexham style at all? Why did not Acca's men or their direct successors introduce a figure or a plait into their elaborate designs? They were evidently meant to be as complete and as rich as their art went; in size and labour spent on the work nothing was spared, but these motives which would have added so much to the interest were absent. Would the early carvers of Hexham have refused the figure-ornament and knotwork which might have made their crosses more interesting if they had known them for possible subjects, as their followers, even at Hexham, did use them later?

Of monuments not directly in the Hexham tradition, the most primitive in style is (14) the cross at St. Andrews, Auckland, with its great base, now in fragments. The figure-drawing is nearest to that of St. Cuthbert's coffin, and the execution is elementary; the detail laborious and naturalistic. The base has on one side three saints in very low relief, and on the other side one saint in higher relief but very

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flat, all ungainly and with big clumsy hands, in the conventional Anglo-Saxon attitudes. The shaft has all the appearance of naïve beginner's art; and it cannot be understood but as the work of one who, having seen or heard of Acca's cross, tried to surpass it with fresh motives, introducing figures and bird-scrolls but no plaitwork. Compared with the other great figure-crosses, it is obviously early. The dedication to

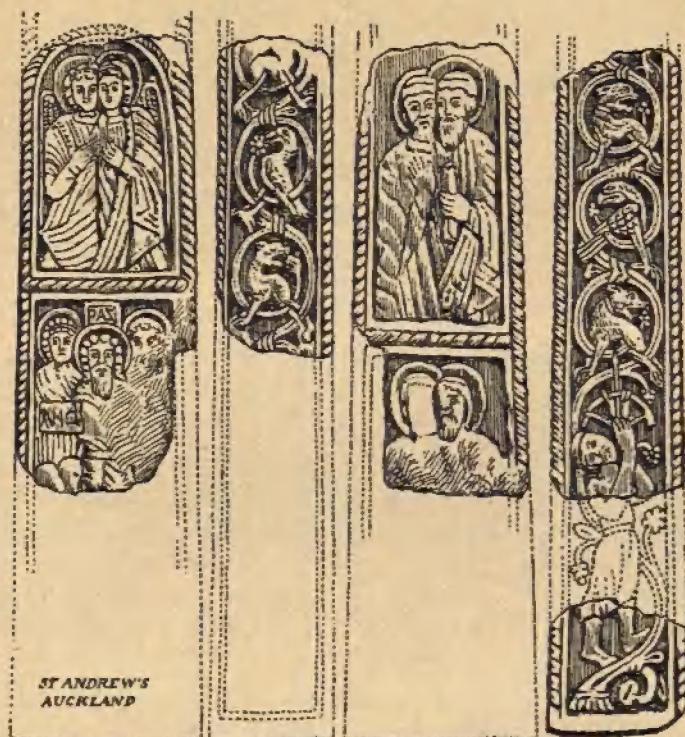


FIG. 2. Cross-shaft, Auckland ($\frac{1}{2}$)

St. Andrew, if ancient, connects the site with Hexham, the home of the St. Andrew cult: and it might therefore possibly be the work of one of Acca's group of men, independent of those who made his cross. I take it as the first of the Anglian figure and bird-scroll crosses, made not long after Acca's, but for reasons already given, certainly not earlier. Put the Acca cross at 740-745 and this soon after, say 750-755, and the others might follow rapidly when once the vogue set in. There is no need to postulate long time for development, if the setting up of crosses became a fashion in this limited area of Northumbria. (Fig. 2).



FIG. 3. The Otley Cross : shaded parts original, outlined parts restored ($\frac{1}{12}$)

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15. A good while afterwards, because time must be allowed for the maturity of its technique compared with the last (14), is Otley Angel-cross. It is comparatively late, one of the proofs being that it had spaces left for lettering, as very early crosses had not. The animals are drawn with more freedom than at Auckland, and the figures show very much more accomplishment, though it contains no panel of plait and therefore has no sign of the 9th century. I put this about 800 and suggest it as the work of someone following Hexham scroll traditions but not working at Hexham. (Fig. 3).

16. The cross once at Easby appears to be later. It has elaborate plaits, and scrolls passing into plaits in a very advanced fashion—an eagle, finely drawn in scroll-work, and a *majestas* of almost classic completeness and accomplishment. There is nothing more ripe in cross-sculpture. It is the best kind of work before the tide turns to degeneracy approaching the middle of the 9th century. The Easby carvers seem to have been trained in a school akin to that of the Bewcastle designers, wherever that was; and the work is not pure Hexham, though it must be partly of that tradition. We may inquire where the school was that could produce it, and I venture to suggest that the artists were trained at Hoddam.

THE HODDAM SCHOOL

There must have been a great workshop of sculpture at Hoddam, sprung from Hexham. From Hoddam itself are four examples (figs. 4, 5; not now there) of Anglian stone-carving of this best period, as well as others of much later style, showing subsequent continuance of output throughout the 10th century. Also Ruthwell is near the abbey and no doubt employed Hoddam men, and the Bewcastle cross stands only 25 miles away, in the same ancient district; there was nothing to prevent workmen from the one place working at the other. The Langbar stone, if intended for Bewcastle, seems to hint that the carving of that cross was locally done by men travelling to their job.

17. Of Hoddam remains, the stone with the 'doll's house' motive now at Edinburgh is somewhat uncouth and yet not very early as crosses go, because of the large space left for inscription—a trait of comparative lateness. It resembles the Anglian stone at Heysham, a work of the Lancaster group, with this 'doll's house' motive but with scrolls of distinctly Hexham origin and with rather late plaits, *i.e.*, plaits of 'easy' construction. These two sculptures must have been

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made under the same influence, and hint that Hoddam was in connexion with Hexham, where the earlier scroll-type arose. That is to say, Hexham men taught Lancaster and directly or indirectly influenced Hoddam. (Fig. 4).



FIG. 4. The 'doll's house' cross from Hoddam ($\frac{1}{2}$)

18. Cross B of the three crossheads restored from Hoddam fragments (formerly at Luce) also has the open flowing scroll seen at Lancaster and Heversham, betraying the presence of some of the group of men who came originally from Hexham and worked both at Hoddam and Lancaster.

19, 20. But Hoddam heads A and C, while connected with the last by a central figure instead of a mere boss, are a new type; and the form of the cross-head ('spatuled') is that of Ruthwell as it was originally. (Fig. 5).

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21. The Ruthwell cross can hardly have been made by artists other than those who worked at Hoddam. Question has been raised as to their origin: these examples, set side by side, settle it. The Ruthwell cross is not Hexham work, but derived in part from Hexham through the school of Hoddam at an interval of a couple of decades. (Fig. 6).

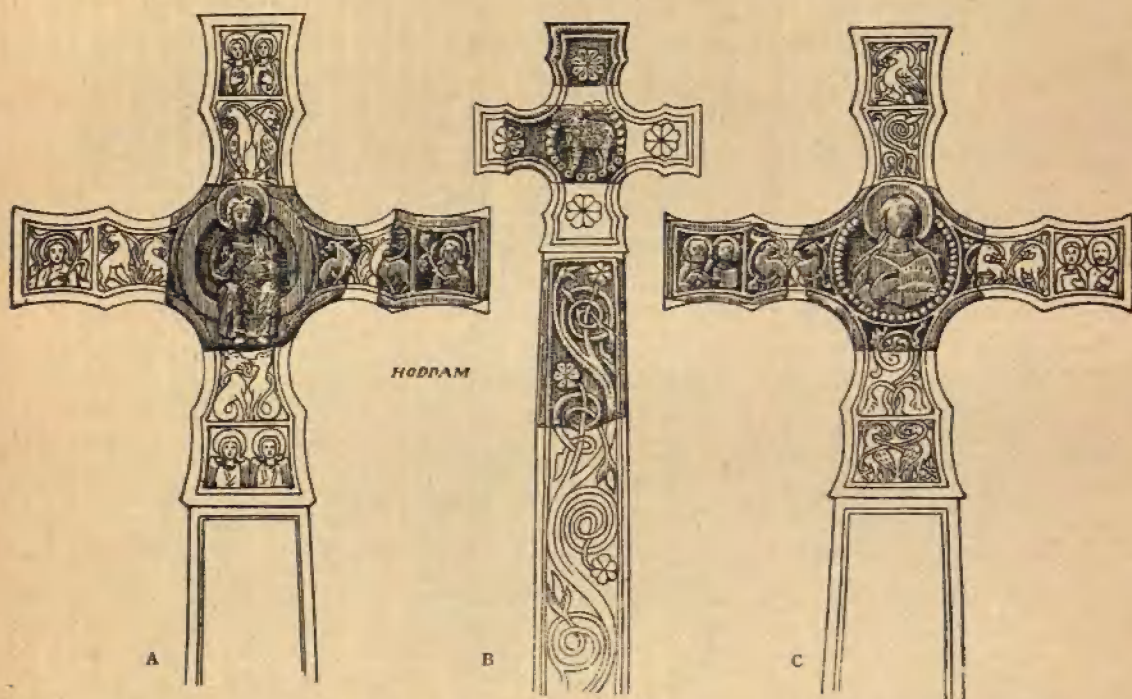


FIG. 5. The Luce cross-heads from Hoddam ($\frac{1}{12}$)

22. And if Ruthwell cross was made by Hoddam men, so must Bewcastle have been, only later—later because it is more mature and because at Ruthwell no spaces are provided for lettering, which was added after the cross was complete, while Bewcastle has (like the 'doll's house') a big panel left blank for the purpose. In other ways it shows a much greater number of motives—plaits of advanced types, the chequers and the dial; while the scrolls, though more elaborate and very florid, are still ultimately of Hexham origin, as shown by their interlacing at the crossings of the loops. The Falconer, too, as A. S. Cook pointed out, is not likely to be before late 8th century. In fact,



FIG. 6. Ruthwell Cross (1/2)

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I do not see how it can be placed earlier than the end of that century ; it is a belated memorial to Alchfrith and his family (see *Northumbrian Crosses*, p. 116 f.). (Fig. 7).

This brings us down to about the beginning of the 9th century. In the past 60 years we have seen a small number of workmen at Hexham, Hoddam and Lancaster as their chief centres, and perhaps other places, bringing the art of the monument to such perfection as it attained in the early Anglian (pre-Viking) series of which we have the remains of less than 20 examples, datable before 800. (The numbering above includes 10-13 which are later pieces, though of Anglian style, and therefore included in the list). No doubt once there were more, but I cannot trace remains of more than these to attribute to this early period, 740-800. That being the case, the output and the progress of development were not too great for the time allowed. After about 800 we begin to see the effects of a somewhat different school, in work often on a smaller scale, highly refined and of florid design, some of which can be attributed to Ripon.

THE SCHOOL OF RIPON

The pieces left now at Ripon are (A) the cross-head (fragment) delicately carved, with chevron border and triquetrae on one side, chevrons and 'lorgnette' (superimposed crosslet) on the other, and a knot at the end of the arm ; (B and C) two interlaced stones (built into a buttress at some late restoration) with 9th century knotwork, *i.e.*, parts of a cross-shaft ; and another (D) head of 10th or 11th century type. At Kirkby Hill is the impost of very fine plait and scroll, apparently from Ripon, but built into an 11th century doorway here, from a door of earlier date.

These seem to give a clue to the source of a number of 'lorgnette' and chevron crosses as deriving from Ripon, namely Northallerton, once an important monument (fig. 8); Hornby with the 'loaves and fishes', very delicate carving; Carlisle, a fragment of chevron head, less finely carved but having the Ripon motives; and another at Heysham, where the Lancaster-Hexham style had not a monopoly.

To judge by delicacy of carving, which is certainly a Ripon characteristic, a few may be added as works presumably of the school. The Otley dragon shaft with its late plait and creatures tail to tail ; the Croft fragment, very refined in cutting but certainly late in the Anglian age by its plait and upside-down animals. Here we have perhaps the first examples of the tree scroll, along with a pretty scroll

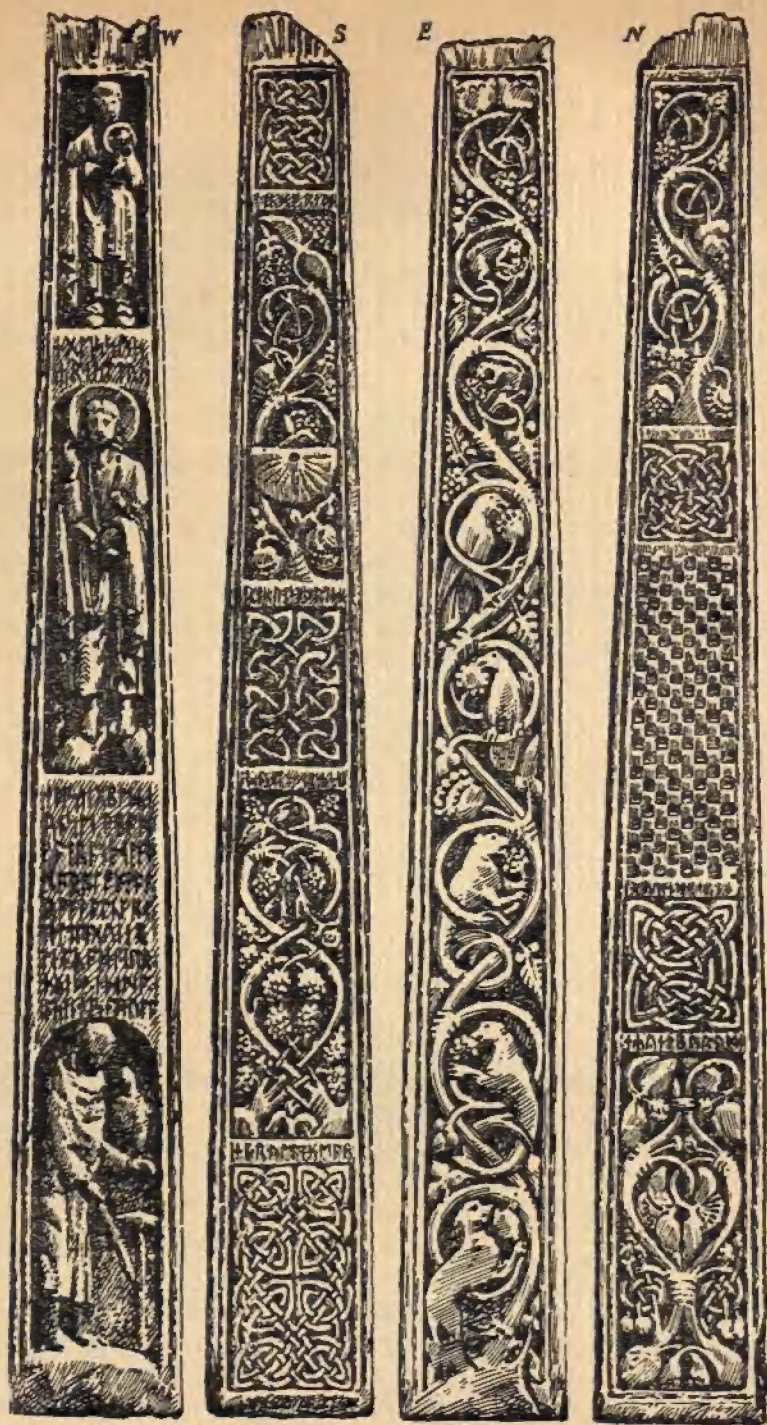


FIG. 7. Bewcastle Cross ($\frac{1}{2}$)

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distantly derived from Hexham and like that at Kendal on which three twigs meet inwards, almost forming 'late Celtic' trumpet pattern. Add to these the Dacre lion, also very delicate, with unusual key-pattern; and the two recumbent grave-slabs at Melsonby—these all are of mid-Yorkshire origin in the 9th century. To these crosses add the neighbouring and coeval stones at Tanfield, Wensley, West Witton, Wycliff, Gilling West ('lorgnette' head on a round shaft), all of which

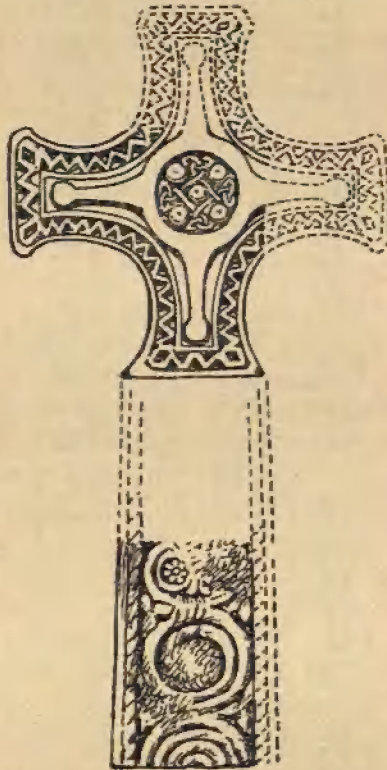


FIG. 8. Northallerton Cross ($\frac{1}{12}$)

are minor works. The great crosses of Masham and Cundall are connected with Croft by the grotesque drawing of their animals, not inelegant but heavy-necked. Masham has a great round shaft to which no doubt belonged the fragment of a large head (now in the church) of florid scrolls and knotwork: and the lower tier of the shaft faintly resembles Cundall (parts formerly in Aldborough Museum), in having the heavy-necked beasts. The beginning of this ungainly beast with

A PEDIGREE OF ANGLIAN CROSSES

heavy neck is traceable in Ruthwell and Bewcastle crosses, enough to suggest affiliation of the (Hoddam) carvers with the subsequent (Ripon) school at no very long interval.

The Hovingham slab of the Annunciation and other figures under arches and a good bird-and-beast scroll is closely like the work at Masham and therefore of the Ripon school, early in the 9th century.

LATER YORKSHIRE CROSSES

Hackness cross, by its late scroll and plaits, cannot date before the third quarter of the 9th century. Another indication of its lateness is the double outline to the confronted beasts, seen also at Crofton. They are in the mid-Yorkshire tradition, distantly descended from Ripon, which may have been the source of many minor groups of carvers, such as those who migrated north and after the Danish invasion worked in county Durham, Northumberland and the south of Scotland (Jedburgh, Closeburn and Thornhill in Dumfriesshire). (Fig. 9).

As a late work of a carver of the Ripon tradition (*i.e.*, end of 9th century) I should class the Ærswith cross at Collingham, which has one motive of Cundall distinctly repeated in the beast with head stooping down between upraised forelegs. And if so, the Apostles cross at Collingham, round-shafted when complete (like Masham, only smaller), is of similar date and tradition. The whole Ilkley school can be connected with it, as inspired from mid-Yorkshire though becoming an independent group. It was not foreign work or the importation of fresh ideas that produced the monuments of Ilkley, Dewsbury and Thornhill (Yorks). They derive in a straight line from the Yorkshire school.

At York itself we have only minor and later remains. Adhusa's cross (Hospitium 13) is a 'lorgnette' known to be from Ripon, but quite late. The head fragments (Hosp. 11, 12) are of fine late Anglian work and form. No. 11 is 11th century by its unornamented surface, and no. 12, inscribed 'Salve pro meritis' etc., is in that respect similar. So the 'Two Gentlemen' (Hosp. 9) have a coarse late scroll, and the stone (Hosp. 21) with the unusual knitting-stitch and long-necked beasts. All else at York is later still. In fact the great city of York does not seem to have had a monumental style of its own in early Anglian days: or if it had, the whole of such remains perished under the stress of the Danish attack. And yet it is hardly likely, considering what happened elsewhere, that no trace should be left, if there were any actual remains.



FIG. 9. Attempted restoration of the Hackness Cross (17)

A PEDIGREE OF ANGLIAN CROSSES

OTHER LATE SCHOOLS

To return to Lancaster and the south. At Lancaster itself a school of carvers evidently flourished continuously up to the 11th century. Some of their work we see at Halton on the Lune, in the fragments of a tall cross with the flock of sheep and the Angel of Remembrance. This last motive is like one in the Otley Angel-cross, and it was copied in a second cross at Halton, on which the tree-scroll and the plaits are not earlier than the end of the 10th century. Another fragment betrays a curious overlap with the Ringerike style of the early 11th century, leading to the 'Sigurd' shaft which shows an extraordinary survival of the same old Anglian ornament blended with Norse of the age of King Knut. Thence onward we find, in Lancashire and far south—through Whalley and Sandbach, Bakewell and Sheffield, Eyam and Derbyshire generally, Northampton and Notts, and even to Cropthorne in Worcestershire—a series of remains, Yorkshire-Anglian in ultimate origin but Ringerike-Norse in character, which seem to be the descendants of this school in the early part of the 11th century—not independent creations but affiliated to the movement which had died out long before from its first home in Northumbria.

Parallel but distinguishable developments went on in southwest Yorkshire and in the north. In Yorkshire we see the school of Dewsbury and Thornhill growing out of Ilkley and therefore connected with the 9th century Yorkshire art, but specializing in plaitwork of a non-Danish type (at Thornhill), but certainly later than the Danish invasion. It is plain that when the Danes settled in Northumbria they settled only on the east of a line drawn roughly through Leeds north to south. There is no trace of the Danish Jellinge work (first half of 10th century) elsewhere, though it is common in that district; but the old kingdom of Elmet (roughly the West Riding) remained Anglian in culture until about 950 when the Norse settlers filtered in from the west, and Scandinavian design mixed with the older English which survived in many places; for example, in the neighbourhood of the Walton cross, where the main element is debased Anglian but tinged with Ringerike, not Jellinge character.

In the 10th century there were in the north two principal schools, that of Hoddam and that of county Durham. From the first came some late Anglian-style crosses in Cumberland, as at Irton, and in Dumfriesshire (Closeburn and Thornhill being of Hoddam style). From the second very many late works clearly descended from Hexham style (some already named at Simonburn, Falstone, Jedburgh) and

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south Scottish pieces (Abercorn and throughout Lothian), in districts where the old Anglian population still inhabited or descendants of the carvers driven out by the Danes still found employment.

These by slow gradations became Celticized, and the well-known Scottish crosses were the result of a movement begun far to the south, but changing character as it found itself in the new surroundings. Ultimately it became the Pictish school of the 11th and 12th centuries, parallel to the developments of that age in Wales, Cornwall and Ireland.

Viewed in this way we can see a perspective of development perfectly logical and flowing without a break from Acca's cross as progenitor. I confess that my pedigree has no room for the Reculver stone as Anglian of the age before 700, but I do not admit that the series presents difficulties. There is no undue hurrying of the development in taking 740 as the start, because for the first 60 years we have only to find room for a score of works.

One more remark arises from criticisms made by supporters of the earlier dating of these crosses. The idea that the political high-water-mark coincided with the zenith of art is an illusion; it is contradicted by the art-history of every style and school. It is after the maximum of national energy has been displayed that art is at its highest, and that was the case in the 8th, not the 7th century, in Anglian Northumbria. It was not in the time of Oswy's (Oswiu's) victories, still less in that of Oswald, that arts and crafts flourished. There was then enough to do in the struggle for life. It was at the time when bishop Albert was building and Alcuin was teaching, even though the dynastic rivals were disputing the right to rule and the Vikings were beginning to nibble at the fringe of the kingdom. The bulk of Northumbria was prosperous and wealthy till long past the time of Bede, and it was then that refinement and the interest in luxury and arts began to show their effects. Until the turn of the 7th to the 8th century, the Anglians were rough and rude; civilization came late to people who in 600 were boors, and in 500 barbarous. The outburst of cross-carving could not have taken place so early as the life of Alchfrith, when Wilfrid was the pioneer of culture in a rough borderland such as Northumbria was during his reign. The attempt to date the rise of art to this age breaks down when the circumstances are better understood, and that is why the present writer has been gradually and reluctantly forced to modify the views in which he was brought up by teachers of a past generation, who dated Bewcastle cross to about 680; that is why he is now led to place it at the more reasonable date of a century later.

This Fieldwork

by R. E. M. WHEELER

IT is better to dig than to dance', said St. Augustine. This sober choice between two difficult alternatives is one which, I freely confess, had always appealed to me as that of a balanced and discerning mind. Today I am a little shaken in my confidence. It has been brought home to me, with something of a shock, that this *obiter dictum* might in a certain sense be cited as a justification of the (to my thinking) distorted outlook of the modern archaeologist. At the present moment, for good or for ill, digging has, it seems, become to a large extent the basis of archaeological research. I say 'for good or for ill' advisedly. To those of us who may be classed as Antiquaries of the Older Generation, there is something indecorous, almost irreverent, in all this hot and untidy pick-and-shovel work. It is so hearty, so vocal, so undergraduate. Besides, it is so easy. Next to committing a murder, the easiest step to fame nowadays is to dig up a potsherd. And the more ignorant the excavator, the greater the kudos. 'Potsherd dated 543 B.C. found in Kent' may not be a hundred-per-cent. news-item; but 'Mystery potsherd in Medway midden: experts baffled' is a main-page headline, sure thing. It is hardly to be wondered that, in our sound-proof studies, surrounded by our genealogies, our heraldry and our armour, we slippered veterans move uneasily in our chairs. It is as though a fist had been thrust rudely through our window-pane and let a draught of cold, oxygen-laden air into our hitherto inviolate *sancta*.

However, times change and other antiquaries change with them. Fieldwork has presumably come to stay. As I pen my plaint, I have in front of me two volumes, one French, the other German. The former is entitled *Manuel de recherches préhistoriques*, and is a revised and largely rewritten edition (dated 1929) of a work originally prepared in 1905 by the Société Préhistorique Française.¹ It is associated with the names of E. Hue, Marcel Baudouin, Breuil, Coutier, Franchet, A. de Mortillet and others, and may be regarded therefore as the authoritative pronouncement of French field-archaeology. As the

¹Published by Alfred Costes, Paris, 1929. 198 small and sometimes obscure illustrations.

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writer of the preface remarks, 'this Guide will, we are convinced, render the utmost service to beginners desirous of profiting by the experience of their seniors, and of learning in the school of the modern pioneers of the true Prehistoric Science'. Let us for a moment put aside our Papworth and (re-adjusting our cushions) attempt to follow the blazed trail.

In the first chapter we pick up our tools. These include the following: crow-bar, boxes, candles, compass, chisels, notebooks, lifting-jacks, hooks, rope-ladder, string, labels, axe, acetylene lamp, marine lantern, hammer, measuring-rod, blotting paper, packing-paper, cotton-wool, picks (various), pick-hammer, pincers, special knapsack, silicate of potassium, drill, sounding-iron, glass tubes, water-colours, etc., etc. In chapter II we survey the landscape in search of likely spots, and in chapter III, having selected our region, we proceed to put the natives through the Third Degree. Here we first strike that note of caution which recurs like the drone of the bagpipes throughout the book. 'It is prudent not always to trust one's local informants in the matter of an excavation, a depôt or a discovery; their information may be false either through ill-will or because the informant desires to retain his knowledge in order that he may exploit it in his own interests'. Nevertheless, tact and persistence will win through, and the time will arrive when we shall have to approach the landowner. This should be done, 'either in the presence of witnesses or in writing'. More caution! Model 'agreements' are printed to facilitate these arduous and tricky negotiations, and success in the preliminaries is at last achieved. Now for preparations on the site. But oh, what perils do environ! You may fence-in your site with the utmost care; yet 'in spite of all these precautions, one must expect to be robbed, happy indeed if the outrage is only the outcome of the cupidity of a simple workman'. It is little to be wondered at, therefore, that, in engaging our simple workmen, we 'must enquire, before all, into their manual capacity and their honesty'. Remember, too, that '*the best way to ensure the honesty of your workmen is not to leave them a minute*' (author's italics). This affectionate get-together spirit must surely turn a French excavation-party into one great happy family.

Follows a chapter of some interest on the French laws relating to the disposal of objects found on private or on State property—a long and complicated business which need not here retard our progress. Then, before getting down to the actual digging, we are instructed in detail in the all-important task of recording our potential discoveries.

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We begin, quite properly, by marking the site on our maps. We then prepare photographs, sketches and water-colours of the site. Measurements are important: but, since indications written on the picture detract from its artistic beauty (*beauté artistique*), reserve a large enough margin for this purpose. From artistry we turn to geology and thence to the collection of pertinent local folklore. For the latter, it is sad to reflect, we must turn to 'the old people, because they have preserved traditions more faithfully, whilst the young people sometimes ignore them completely'. If there are local superstitious rites, we 'must, if possible, take part in them in order to render an exact account'.

Eventually we approach more nearly to our muttons. Much sound advice is given in regard to the working-record of the excavations. Omit nothing—'note all the incidents of the excavation'. Even so, you cannot be sure that in the end anyone will believe you. If, therefore, there are witnesses of your discoveries, 'beg them to sign the report of every important find'. Take no risks. And, not least, do not forget that pirates are waiting eagerly to secure for themselves the credit of your discoveries. If you cannot publish a note on these at once, you are recommended to deposit an account immediately, in an envelope sealed with five seals, at the Academy of Sciences or the Academy of Inscriptions. Thus only can priority be assured and honours given where honour is due.

But we are not yet sufficiently qualified to sally forth into the great open spaces. In successive chapters we learn how to use a map and compass and to make a traverse; how to preserve objects after discovery (a pretty good chapter, but sketchy in the matter of metalwork); how to make models; and how to photograph. The chapter on photography would have been more illuminating without its illustrations. These consist of a series of foggy blurs alleged to represent megalithic monuments, *in each case* (so far as visible) *without scale*, although the text has all the right ideas on this point. And once again we meet the ghost at the party. 'Be sure to take a photograph at the *actual* moment of any sensational discovery, *including in it the persons present as witnesses*'. The italics are again the author's. How closely akin are duplicity and simplicity! It is without surprise that we are next confronted by a whole chapter on 'the authenticity of prehistoric objects', stimulated in some degree by the Glozel hoax and contributed partly by M. Champion, one of the exposers of that hoax. Chapters on the human skeleton, on craniometry, on pottery (quite useless), and on 'the installation and arrangement of collections' (a matter on

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which France has not a great deal to tell the world) prolong our suspense, until we begin to suspect that our veteran pioneers have lost the way. Our suspicions are confirmed when we suddenly find ourselves entangled in the jungle of an irrelevant 67-page treatise on the whole of French prehistoric archaeology. The obstacle is a serious one, but if we persevere we eventually win our reward. On page 279 appears the blessed word 'stratigraphy', and we at last come to grips with our subject. The excavation of a cave is described at length and with much reason, for in this department of fieldwork the French have won their spurs. And there are points in the section on the exploration of chambered tombs, although in this matter the standards set in this country by Mr W. J. Hemp have rarely, if ever, been approached in France. But for the rest, even from the study-window it is easy to see that France has little to offer. An entirely unhelpful section on the excavation of hill-forts merely affirms vaguely that 'the excavation of a fortified enclosure should be carried out with the greatest care and the greatest observance of method'. What that method should be is not indicated. Another section, on the investigation of tumuli, advocates a system of pseudo-scientific mutilation that has been obsolete since the days of Pitt-Rivers. The whole book is a shoddy and pitiful display for the combined talent of France. We are left throughout with the impression that, at the best, the captain is polishing the brass-work and arguing with the cook instead of navigating the ship. It is inconsequent, directionless, and—un-masculine. Wake up France!

With this trumpet-call, we turn on our cushions more hopefully towards the German volume. Superficially it might seem to be the equivalent of the French handbook, for it is entitled *Grundfragen der Urgeschichts Forschung*.² A glance within, however, reveals a different world. Here at last is unity of command and definition of purpose. The word Method occurs significantly no less than three times on the first⁷ page, and all is indeed set forth in disciplined array. We stand rigidly to attention whilst the human sciences—Anthropology, Philology, Archaeology—goose-step past us. This, we feel, is as it should be. The author, Dr K. H. Jacob-Friesen, is the director of the Hannover Provincial Museum, and his book is an orderly museum-book. In it the multitudinous and inexact science of humanity is given simplicity and precision, is epitomized, labelled and docketted for easy reference.

²Published by the Helwingsche Verlagsbuchhandlung, Hannover, 1928. 240 pages, 18 illustrations, mostly small distribution-maps. Mk. 20.50.

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The epitome is overwhelmingly Teutonic, as such epitomes are apt to be ; but the author occasionally breaks his frontiers—is aware, for example, that a certain *Crawford* has plotted archaeological distributions in the Wash-Busen, and that one *Cyrill Fox* has produced some instructive maps of Cambridgeshire. We may describe the book, therefore, as reasonably comprehensive, and may at the same time congratulate the author (and ourselves) on its conciseness. In these and other respects his work has many merits that are absent from its French contemporary.

But in detail, the two handbooks are not comparable. Dr Jacob-Friesen's underlying purpose is, perhaps, definition rather than detailed instruction. He sets the three sciences side by side, summarizes successively the main contributions of the more notable (German) scholars in each of them, tabulating their work by subject or by method ; and finally attempts to determine the proper scope and significance of each science, with a view to an ultimately closer co-operation between them. All this has a rather theoretical and abstract tendency, and the whole book is definitely of the arm-chair. Nevertheless, not a little of the incidental matter is of relevant interest. The section dealing with archaeology (' *Kulturhistorische Forschungen* ') includes sub-sections on Distribution, Morphology, Chronology and Method (touching lightly, very lightly, on fieldwork), together with resumés of theories relating specifically to the early history or prehistory of the ' Indo-Germans ', the Germans, the Celts and the Slavs. All this is very succinct and, up to a point, informative. One is left with the general impression that the book is useful. But useful to whom ? It is difficult to say. Perhaps to university honours-students, or to museum-curators ; anyway, to people who are generally too busy acquiring information to bother unnecessarily about education. Be it repeated, however, that the book is, of its kind, a good one and pleasantly produced.

And now we have suffered long enough the elbows of those jostling archaeologists whose unseemly boast is that they labour ' *à la manière du Géologue et du Zoologiste, c'est-à-dire à la façon des Naturalistes* '. Forsooth ! It is but a step to that knowing scientist who has computed that Man is but five-shillings' worth of chemicals. Let us gather our coat-tails about us and return thankfully to our brass-rubbings.

Museums Old and New: some personal impressions

by O. G. S. CRAWFORD

MOST readers of *ANTIQUITY* will have heard of the Pergamon Museum at Berlin; its opening was the chief event of the centenary celebrations of the German Archaeological Institute (see Mr R. G. Collingwood's note in *ANTIQUITY*, 1929, III, 339). Those, however, who have not actually seen it can hardly realize what an outstanding achievement that museum already is, even in its present unfinished state; nor perhaps do they know that the Pergamon reconstitutions do not stand alone. There are similar reconstitutions of other classical buildings and also of Middle Eastern remains. About five large rooms are at present opened, and their contents represent the high-water-mark of museum-craft in Europe,¹ and are an object-lesson to the world; but to me their significance seemed even deeper, marking the triumphal emergence of a new craft. I have seen many museums, but here for the first time in my life I felt completely satisfied. The ideal aimed at seemed to have been achieved; there was nothing to find fault with. Here at last was a thoroughly honest attempt to reconstruct ancient masterpieces of architecture, and to exhibit them without irrelevant distractions. The technique of exhibition achieves its end through simplicity and common sense. The lighting is admirable; the floors of marble slabs (pink in the Altar-room and white or grey elsewhere) are thoroughly pleasing; the walls are delightfully bare, with nothing but the briefest of labels carved in admirable lettering. The effect of the whole is over-whelming.

Why?

Because for once one is put through direct to the past with neither interference from the operator nor atmospherics from the environment. Here, one feels, the authorities are trying to obscure themselves. They

¹ I am concerned only with Europe in this essay. Having never been to America I can say nothing at first hand about the splendid museums there.



THE PERGAMON ALTAR, AS RESTORED IN THE PERGAMON MUSEUM, BERLIN
Ph. Pergamon Museum, Berlin

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want one to appreciate a work of art as its creator intended ; and that after all is how one approaches the past nowadays. One had come across valiant attempts of museum curators, faint but pursuing an ideal, but never success on so magnificent a scale. To have conceived and been able to execute a design like this betokens a great artist.

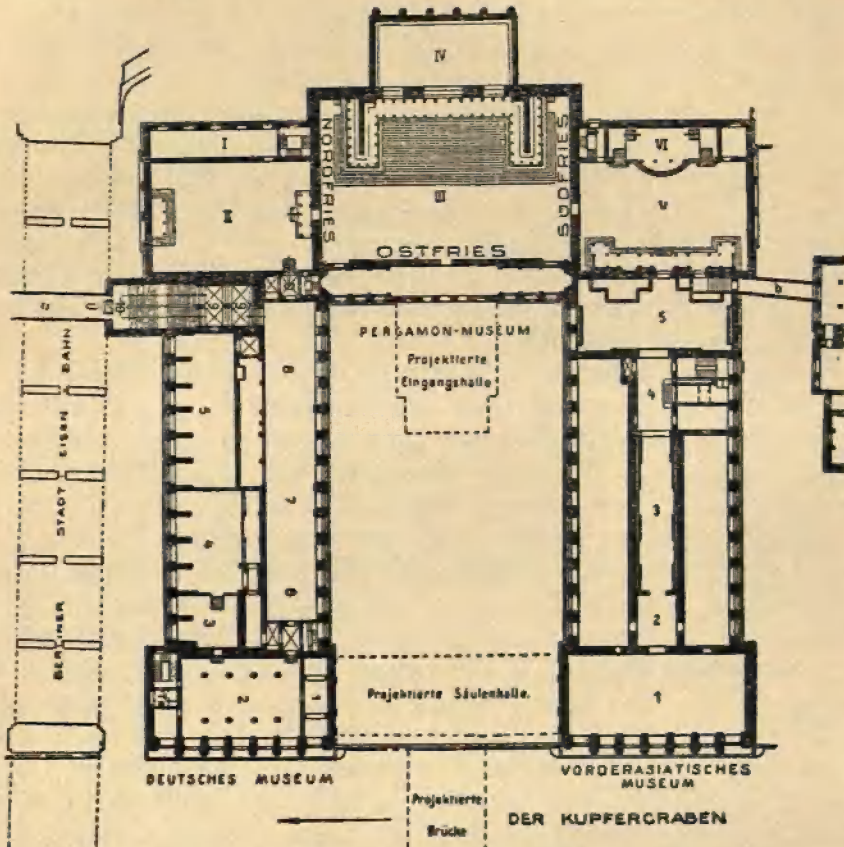
The whole setting is typical of the modern attitude, as I have said. Nowadays whether we are interested in prehistoric man, or the South Sea Islanders, or medieval serfs, or Tuscan artists, we endeavour to understand them ; we try and find out how they looked at the world, how they dealt with the fundamental problems, and the minor problems, of existence. In a world of ideal Museum Directors and overflowing treasuries we might expect ideal museums. In the world as it is we seldom find them ; and it is most significant that the brightest example should have been created by one of the youngest republics in the darkest hour of its existence.

Formerly, of course, the view-point was quite different. The 'great ones' of the world used the masterpieces of antiquity as an offset to their own magnificence ; and interest in humbler things had its roots mainly in a Pharisaic smugness, contemplating the heathen in his blindness. We have got rid of that, but we still do not realize that the conversion of palaces into museums, though certainly in one sense a step in the right direction, was still only the first step. Palaces are themselves relics of the past ; the purposes for which they were built and the pretentious arrogance of their whole design are both utterly foreign to the spirit informing, for example, the Pergamon Museum. This new spirit, which is simply a manifestation of the scientific spirit, looks forward not backward, even when it is dealing with the past. It is not at home in palaces, the cast shells of a bygone régime. For guidance in action it looks not to tradition but to commonsense and the specific needs of the case. To the architect who is designing a museum, both imitation Gothic and imitation classical styles should be equally irrelevant and abhorrent. He has certain requirements of space and lighting and such like to carry out in a certain material (ferro-concrete usually), and that is all he need bother about.

In mere size the Pergamon Altar-room (pl. 1) is impressive. It is 47 by 30 metres in dimension, and the height is about 18 metres. The roof (seen in plate 1) is of glass set in an iron framework. Both walls and floor are absolutely bare, except for an inconspicuous desk where are sold some superb photographs (from which these illustrations

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are reproduced) and descriptive literature of both popular and learned character. No attempt is made to restore missing portions of the statuary, but such structural replacements as are necessary have been



Plan des Pergamonmuseums

- a. Übergang zum Kaiser-Friedrich-Museum
- b. Übergang zum Neuen und Alten Museum

added, in very good taste and in accordance with the best modern principles.

From the Altar-room (III) one passes to the Roman Hall (v), whose chief objects are the Markt-Tor of Miletus (plate III) and part of the upper portion of the Mausoleum of Carfinia; on the floor is a

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mosaic from a Roman house at Miletus (plate iv). The other objects include a couple of columns from Baalbek (seen in the middle of plate iii).

We pass out westwards into the Near Eastern Museum which occupies the southern wing and is divided into twelve rooms, of which only three are as yet open. We enter room 5 first, and see opposite us a reconstitution of the façade of the Parthian palace at Assur (about A.D. 200), the outcome of excavations conducted in 1911-12 by Dr Andrae. The whole effect is not unpleasing, though the architectural proportions might be better (plate v). It is obviously reminiscent of the Roman triumphal arch. As a link connecting the genuine old-Mesopotamian and the later Hellenistic architecture with Sassanian and early Mohammedan, it is obviously of great importance.

Thence we pass to another great achievement—the reconstruction of the Ishtar Gate at Babylon (plate vi) and of the Procession-way leading up to it (plate vii). Here the element of colour adds greatly to the effect produced, and the composition of the whole design is quite well done. A well-lit model (plate viii) and a plan hung above it enable one to grasp at once the place and function of these reconstructions in the ancient city of Babylon itself. A student who has spent half an hour or less here will go away with a clear-cut, permanent mental picture of one of the great monuments of antiquity. How many other museums achieve this? How many try to?

The last of the Berlin museums to be mentioned is the Tell Halaf museum (Franklinstrasse 6, Berlin-Charlottenburg). Here, in an old factory, are exhibited the sculptures and other objects excavated at Tell Halaf by Max Freiherr von Oppenheim, in the years 1911-13, 1927 and 1929. The building is admittedly unworthy of the treasures it contains, barbaric though their style may be; and one hopes that one day when prosperity returns to the world, they may find a better home. The indomitable pluck and perseverance which after 22 years of tribulation produced the Pergamon and Near Eastern museums will certainly triumph yet again over difficulties that may now seem almost insuperable: for nothing less than a whole new building, specially designed, will suffice.

In a future issue we may be able to give a further account of Tell Halaf. For the present, since we are concerned rather with the technique of exhibition than with the exhibits themselves, it must suffice to say that Tell Halaf is an ancient mound-city on the Khabur, midway between Aleppo and Mosul, lat. $36^{\circ} 47' 40''$ N, long. $40^{\circ} 6'$ E. It now

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lies within the mandated area of French Syria, and the sculptures here exhibited consist of both stone originals and plaster casts. In addition a few objects are exhibited, and also excellent plans of the site and photographs of the excavations.²

Let us now go to the most famous of all museums, the Louvre, and institute comparisons. Let me make it quite clear beforehand that I am criticizing *style*, *not persons*, least of all the distinguished conservators and their staff, to whom incidentally readers of ANTIQUITY have special cause to be grateful. Why is it that, of all big museums, the Louvre, which contains I suppose more masterpieces than any other building in the world, is the gloomiest and most depressing? Why does it seem to radiate an atmosphere of aloofness, almost of hostility?

Is it because it is built in a debased style, designed originally for the glorification of idle and useless people? Judged as a work of architecture by certain standards it is almost incredibly ugly, both outside and in. It may perhaps have been suited to the tawdry purpose for which it was made; but that is no justification, and in any case that purpose has ceased to convey any meaning to the world of today. The people who haunted its corridors have departed for ever; but here their atmosphere and traditions seem to linger.

Inside, several criticisms obtrude themselves. First, there are far too many exhibits, and they are crowded too closely together. One's attention is distracted, however hard one tries to concentrate. Secondly, the architecture conflicts with the exhibits. Thirdly, the defective lighting often prevents one from seeing the exhibits, or from seeing them to the best advantage. For instance the frieze of the

² The following list of references was compiled at the museum :—

a. Ernst Herzfeld, *Arch. Mitteilungen aus Iran*. Band II, heft 3 (June 1930); chronological table giving synchronisms with Zenjirli, Carchemish, Boghaz Kiöi etc. in parallel columns. See also his Appendix in *Der Tell Halaf*, 1931, 225-233.

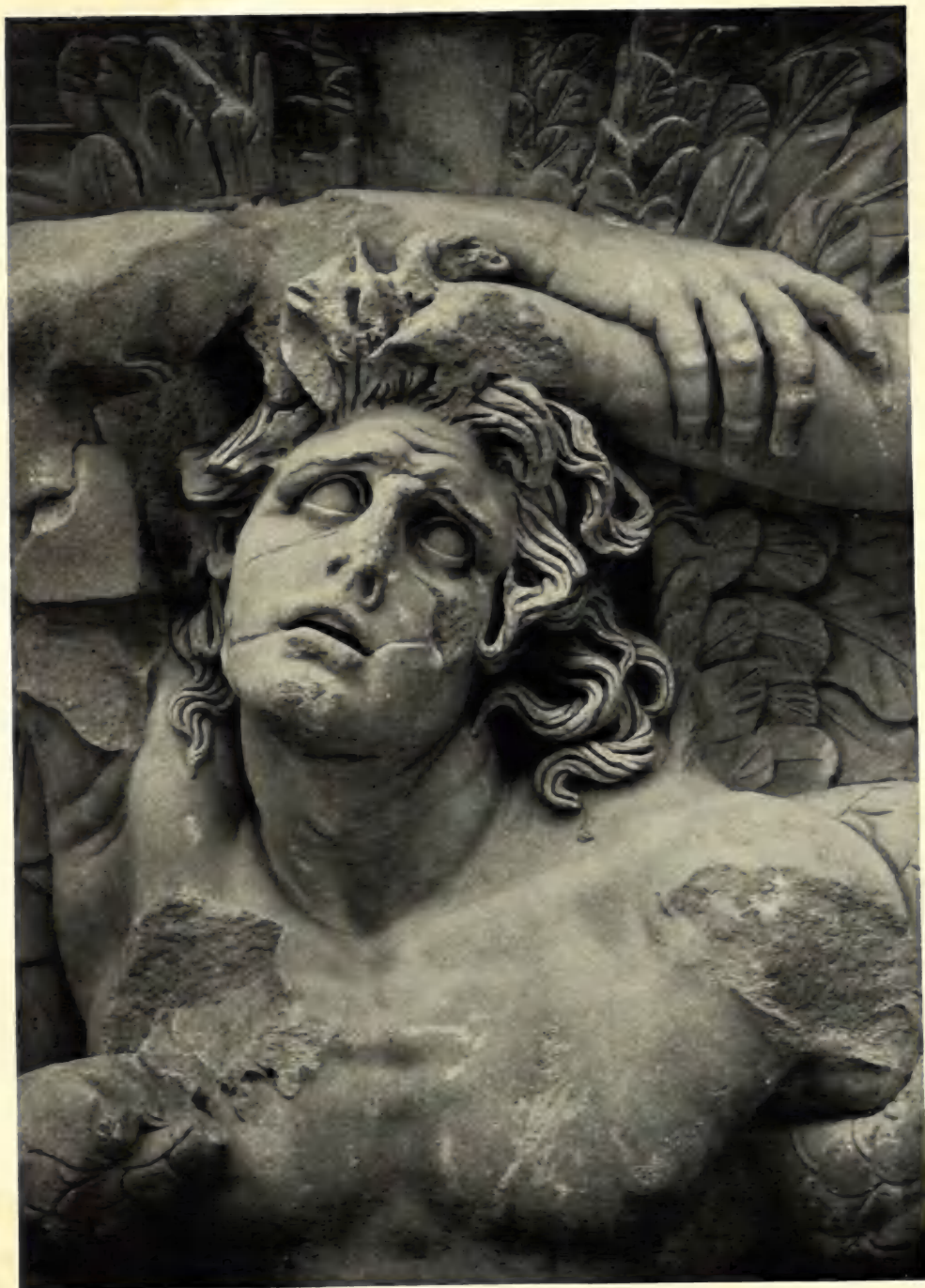
b. *Illustrated London News*, 25 Oct. and 1 Nov. 1930.

c. *Berliner Illustrirte Zeitung*, 17 August 1930.

d. *Pantheon* (Bruckmann, Munich) April 1931, pp. 167-171 ('Die Kunst des Tell Halaf', by A. Ungnad; well illustrated).

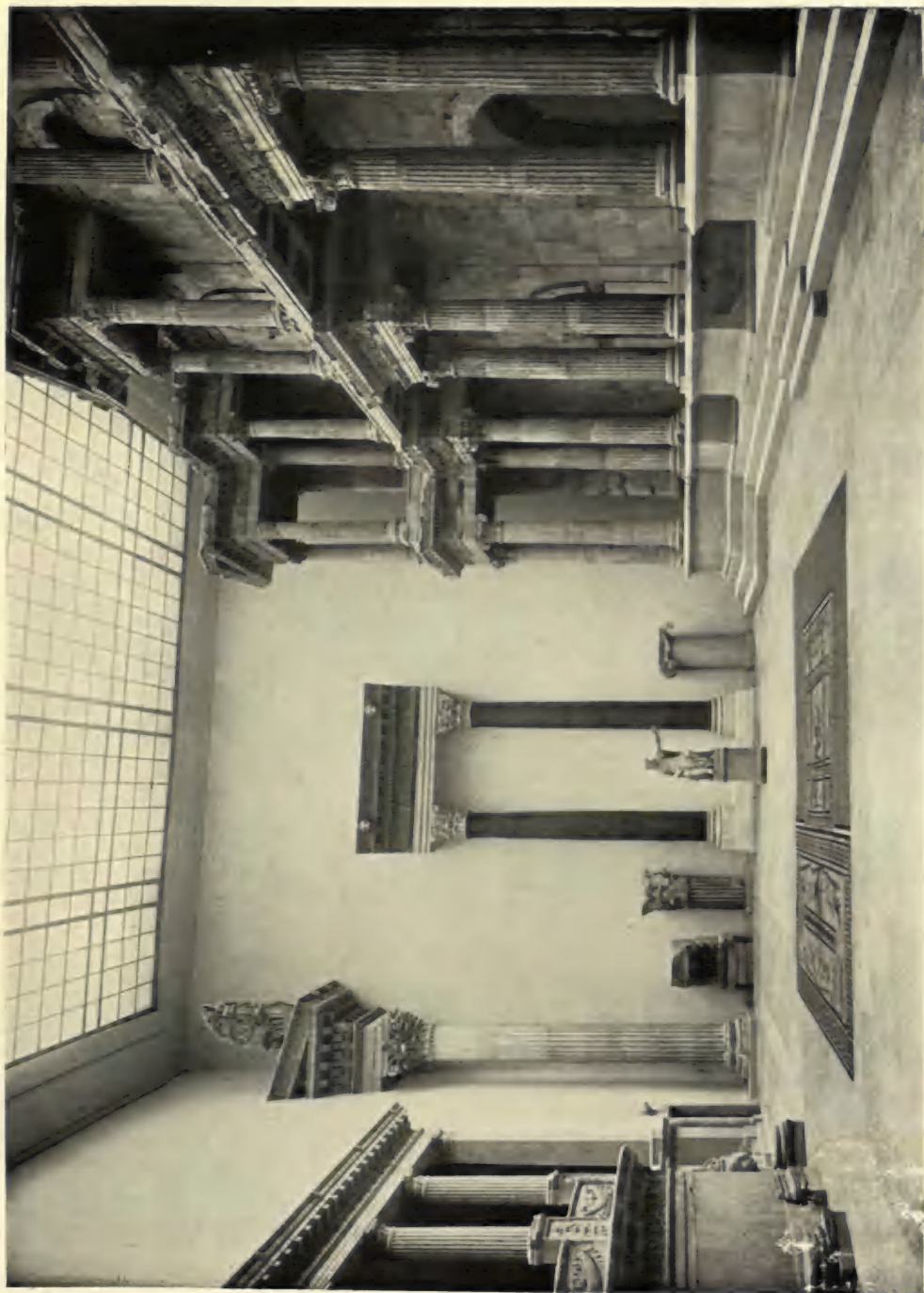
Since then Freiherr von Oppenheim's book has been published :—*Der Tell Halaf* (Brockhaus, Leipzig, 1931). It is not the full scientific publication, but an admirable general account which will be appreciated by both the general reader and specialists. We hope to publish a review of it at an early date.

PLATE II



DETAIL OF SCULPTURE FROM THE PERGAMON ALTAR, BERLIN: ALRYONEUS
Ph. Pergamon Museum, Berlin

facing p. 64



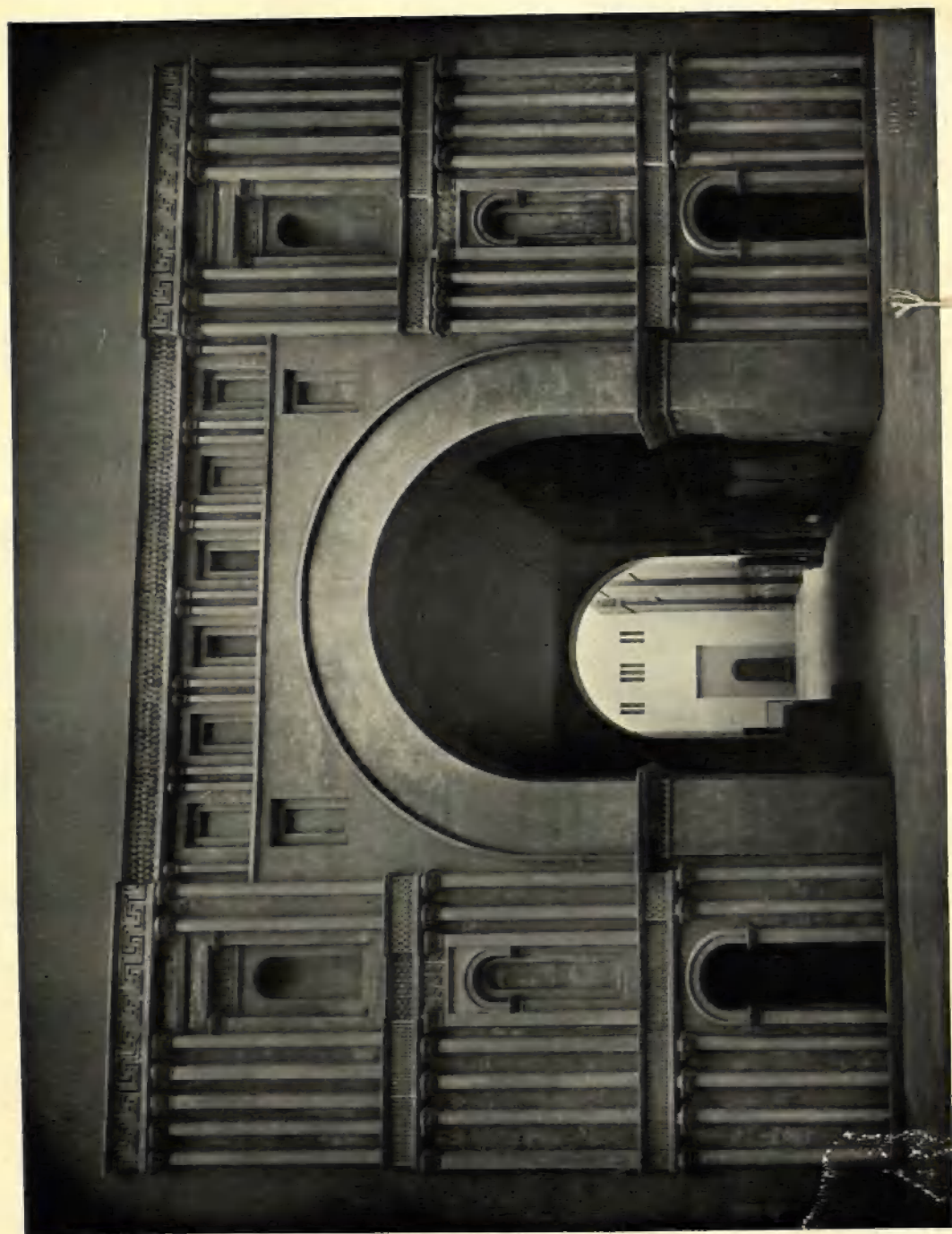
TWO COLUMNS FROM BAALBEK
Ph. Pergamon Museum, Berlin

PLATE IV



PART OF THE ROMAN ROOM OF THE PERGAMON MUSEUM
Ph. Pergamon Museum, Berlin

PLATE V



FACADE OF THE PARTHIAN PALACE AT ASSUR (c. A.D. 200), AS RESTORED IN VORDERASIATISCHE MUSEUM, BERLIN
Ph. Vorderasiatische Museum, Berlin

PLATE VI



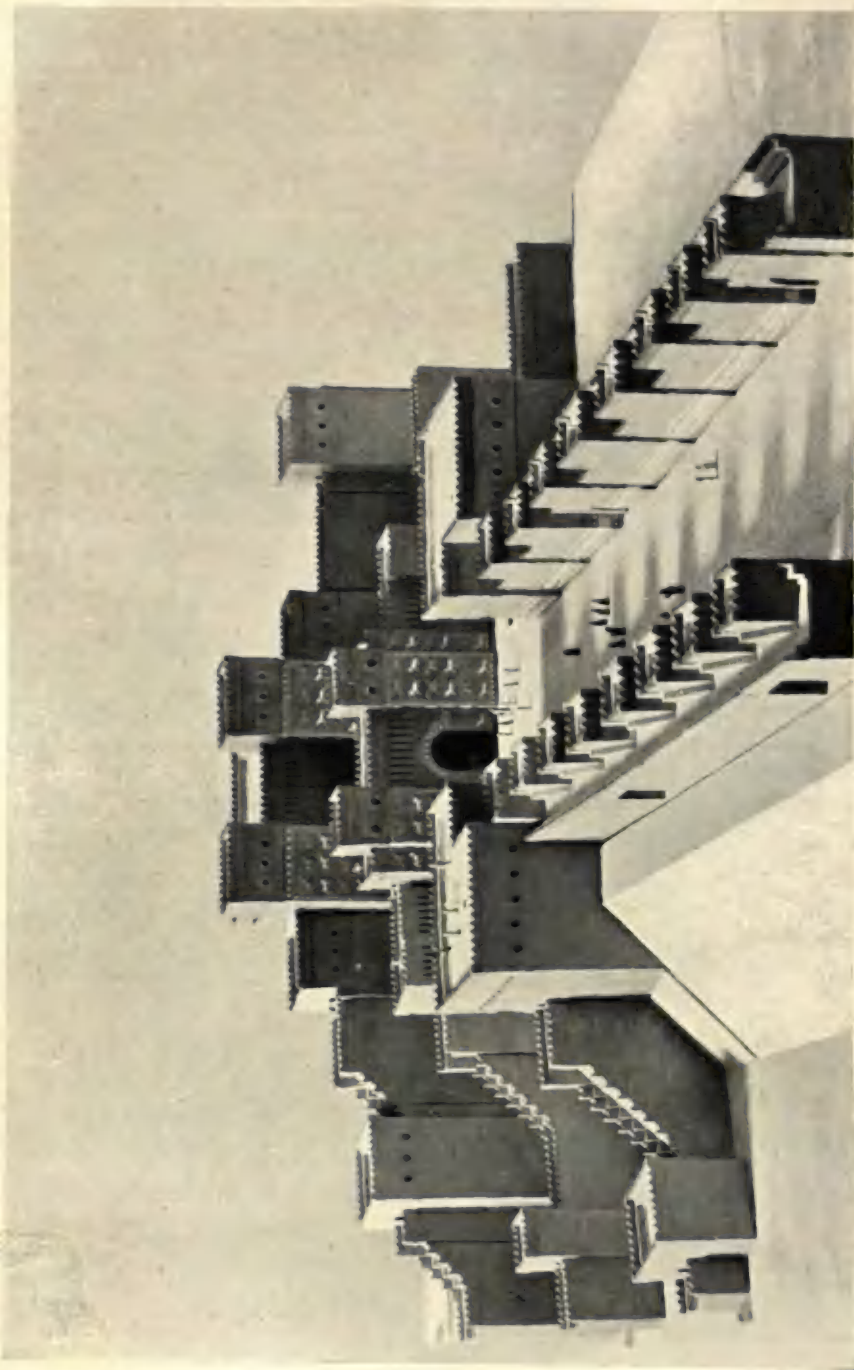
THE ISHTAR GATE AT BABYLON, AS RESTORED IN THE VORDERASIATISCHE MUSEUM, BERLIN
Ph. Vorderasiatisches Museum, Berlin

PLATE VII



THE PROCESSION-WAY LEADING TO THE ISHTAR GATE AT BABYLON, AS RESTORED
IN THE VORDERASIATISCHE MUSEUM, BERLIN
Ph. Vorderasiatisches Museum, Berlin

PLATE VIII



MODEL OF THE ISHTAR GATE AND PROCESSION-WAY AT BABYLON, IN THE VORDERASIATISCHE ABTEILUNG, BERLIN

Ph. Vorderasiatische Museum, Berlin

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Naval Expedition (from Sargon's Palace) loses most of its value because it is placed facing the light instead of at right-angles to it. This is made the more obvious by the adjacent stele of Naram Sin which, being placed at right-angles, is well seen in all its details. The whole of this (Asiatic) room is badly overcrowded. The Assyrian bulls look merely ridiculous. Lastly the labelling throughout, as in so many French museums, is deficient and even when present, unhelpful. The crabbed style of writing on the labels (so common all over Europe) should be definitely abolished.

In the Grande Salle de Suse (Salle VII), devoted to '*antiquités susiennes et perses*' a brave attempt is made to reconstitute two little pylons from the Palace of Artaxerxes Mnemon at Susa, and two friezes of archers from the throne-room of Darius I. Both are artistically pleasing, though their restoration is not altogether satisfactory. They are set against a wall on which some tiresome and quite unnecessary modern frescoes in the Assyrian style distract one's attention. The room is about 100 by 50 feet in size, and about 20 feet high to the cornice. The lighting is bad; there are no plans, models or sections; and the labelling is inadequate.

For all its profusion of art inartistically displayed the Asiatic section of the Louvre is provincial compared with the majestic halls of the Pergamon Museum. Indeed provincialism is the curse of the Louvre, as of so much French work. The heavy hand of the petty bourgeois tradesman is felt everywhere. As one saunters through the stuffy crowded little rooms it is hard to realize that this is the famous Louvre; one keeps wondering when one is coming to the real thing—just as a stranger in a country town might search in vain for the main street where all the big shops are. The illusion of an antique shop is maintained to the end when, at the entrance, one finds a vast hall entirely devoted to the sale of pictures, casts, literature and even of human guides. One is irresistibly reminded of 'all them that sold and bought in the Temple' and their fate. 'Nothing for nothing and damned little for sixpence' is a mean motto for a national museum.

The overcrowding and general unsuitability of the show-rooms originates in the attempt to use a building for a purpose for which it was never intended. Dark alcoves may have been in place in a royal palace, but in the 20th century we have no more use for such things than for the petty intrigues associated with them.

Yet another criticism occurs. The Louvre occupies a huge area

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in the heart of Paris, an area of fabulous monetary value.³ But it does not even occupy the whole of it. To say nothing of the larger western court (Cour du Carrousel), there is a huge inner court (Cour Carrée), covering about an acre and a half, of no architectural or artistic merit and utterly useless. No attempt is made to render it either useful or beautiful.

Destructive criticism—and we repeat our criticism is made wholly in the abstract, without the slightest personal reference—should be followed by constructive suggestion. Having destroyed the Louvre what are we going to build on its site? We shall go to those experts in modern construction who have to meet the demands of modern enterprise—demands for business premises, cinemas and such like. There are already several examples, in Paris, Berlin, Vienna and elsewhere (even in poor old London), of buildings which are both aesthetically pleasing and functionally efficient—the two are inseparable. They are heralds of the dawn of applied science. They are not built in any 'traditional style' because they are themselves the first experimental ventures in a new and living style. Some are perhaps a little crude, because all first attempts are crude. But they are at least unaffectedly honest, unpretentious and alive, and they seem to satisfy, for though expensive they are multiplying rapidly. They represent the application of science to building; and they have their counterpart in those ultra-modern but equally pleasing and equally experimental cottages that were to be seen at the housing exhibition at Berlin last summer. These new ventures in architecture foreshadow a new era, and symbolize the spirit of an age which has cast off authority and outworn traditions. This spirit is rapidly becoming self-conscious, and is expressing itself unmistakably in science and politics. It is still immature and it may of course be submerged or stifled before it reaches maturity. Against it are arrayed the combined forces of tradition, vested interests and inertia. But, in museum-craft (which is the subject we are now concerned with) the ideal has been partially achieved once in the face of these and other even more formidable difficulties. In field-work and excavation science has already triumphed, and now at last it has begun to invade the dusty precincts of museums and workshops. The results will be far-reaching and impressive.

But as yet only a beginning has been made. At Berlin certain

³ The Louvre itself, without its contents, has recently been valued at £400,000,000, according to a report quoted in the London *Evening Standard*, 27 November 1931.

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periods of the past have been isolated and illustrated by means of their architectural remains. This has been admirably done, but there is a vast mass of other material which may be similarly treated. What principles are to guide the selection and display of statuary, paintings, embroideries, and all the mass of 'art mobilier' (such as all that on the upper floor at Bloomsbury)? In a great metropolitan museum, with treasures ransacked from all the ancient civilizations, the only rational scheme to adopt is one which shows the rise and decline of each successive cycle of culture by means of chronological arrangement. Selection will be necessary, and there will have to be drawers, and eventually perhaps whole galleries, and even buildings, for specialists. But this will be a help rather than a hindrance, for the evolution of culture can best be shown by exercising strict economy of exhibits. The surplus, if any, can be distributed to local museums, under adequate guarantees. But this is not the place for the detailed discussion of such a vast problem; one can only indicate certain general principles. Those who wish to read more about this matter may be referred to what I wrote about it more than ten years ago.⁴ On reading it over again for the first time since it was written I see nothing to alter, and little that should be added except in amplification of the ideas there set down.

At this point in my original essay I proceeded to consider the British Museum. It seemed incumbent upon me to do so after criticizing the museums of other countries, however unwilling one might be to perform a thankless task. Since then however there have been rumours of impending changes, and it seems best to say nothing. One would in any case be preaching to the converted. There remain of course other openings for criticism; just as there are many things (such as the laboratory work, special exhibitions, free lectures and labelling) that one would have selected for praise. But the good points are obviously to the credit of the existing staff and their immediate predecessors, and the bad ones just as obviously a legacy of long ago. Consequently one felt even more strongly than elsewhere that, given the means and opportunity, such problems as lighting, arrangement and display would be successfully solved.

⁴ *Man and his Past* (Oxford, 1921) chapter 18. Some severe but justified criticisms of museums will be found in Sir Charles Close's Presidential Address to the Hampshire Field Club on 'The Deadliness of Museums'. It was printed in full in the *Hampshire Chronicle* for 2 May 1931 and will appear in the *Proceedings* of the Club.

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There is one matter however that cannot be passed over in silence. From a most useful handbook⁵ one learns that, in common with all other British national museums and galleries, the British Museum is closed on Sunday mornings. Now Sunday is a day of rest and recreation; it is the only such day in the week for many hard-working people who have a right, as taxpayers, to demand consideration. One knows the real reason of course; but in this, as in other public affairs, there should be a policy of the open door rather than of the protection of vested interests. The practical difficulties of all-day Sunday opening have been overcome abroad, and a change is overdue.

Destructive criticisms of museums as they are in the world of today is easy. Constructive suggestions about museums as they might be in the world of one's dreams are equally easy, but unprofitable. What is required is a concrete adjustment of one's ideal to the world as it *will* be tomorrow, and this is impossible until we enter a more settled period. Certain developments, however, may be indicated as likely to take place during the present or following centuries.

In the first place it seems certain that the 'museum method' will be employed far more widely than at present, and will not be confined to pure science. There will be an immense increase of museums devoted to some special subject or area, or to special aspects of a group of subjects—in other words, of 'specialist' museums. This will leave the field clear for 'popular' museums dealing with the generalizations of science—with for example the evolution of life and of man and his works. There will still remain big metropolitan museums at one end of the scale and small local collections at the other; and the local museum will attempt (as at Newbury and Hanover) to combine width of outlook with the proper display of local culture. But the broad sweep of history and evolution will hardly be fully realized without a visit to one of the metropolitan museums.

Then again I think that museums will acquire souls. At present most of them are dead bodies. A live museum is one which is inspired by the spirit of science. A museum of palaeontology, for instance, should by means of its arrangement illustrate the evolution of life. It should be made absolutely impossible for the visitor to leave it

⁵ The National Museums and Galleries of London: lectures and special exhibitions for October 1931. H.M. Stationary Office, 4d. (Although nowhere stated, we presume this is a monthly publication).

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without learning something about evolution. It should be a teaching institution. The arrangement of exhibits, and of the accompanying explanatory distribution-maps and time-charts, will be regarded as of more importance than the acquisition of specimens. One cannot of course, in a museum, have arrangement without exhibits (though one can have casts); but exhibits without arrangement are a useless encumbrance, and serve rather to kill interest than to awaken it.

It is the old antagonism between the generalizer and the specialist. There is no earthly reason why one should not be both; indeed one is not really a man of science at all unless one can appreciate the full force of generalization as a stage in scientific progress. But instead the specialist often regards the generalizer with suspicion; and he in turn is apt to lump all specialists together as pedants. The one forgets that all generalizations are based upon special research, the other that outside one's own subject one is perforce dependent upon general ideas, general conclusions. In the individual and in the race knowledge advances by means of such generalizations; and the man in the street (who after all pays the specialist his salary) may reasonably demand to be taught the main outlines as well as the details. He can at present learn, if he submits a coin, a brooch or a potsherd, when each was made and where. It should be made easy for him to fit that object into any historical system by a visit to the show-cases. Suppose that the coin is Parthian, the brooch of the Peschiera type, and the potsherd La Tène III b; the historical meaning and content of these necessary terms should be visually displayed there—the culture of the people who made them. If he can thus find out in the showrooms, something about the Parthians—when they lived and where, and what sort of people they were—the coin may have started a train of interest that will fire his imagination and lead him on to investigate further. The interest of most people begins in some such way with something of their own they have actually found or seen themselves. Under favourable circumstances it often develops; but we have known of cases where it has died in a museum, though not always 'from natural causes'.

Such schemes involve building on a large scale. The Pergamon Museum points the way. There the preliminary problem was, one would imagine, straightforward; special buildings had to be designed for certain definite exhibits. The problem of designing a whole series of museums that shall be in alliance with science on the one hand and education on the other is far more difficult. Science we have, and of the best, but have we anything yet that can properly be called education?

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So far as staff and exhibits go we have magnificent raw material which is not always used to the best advantage. We suffer, it is true, from certain disadvantages of climate and traditions, but they can and will be overcome. Meanwhile the formulation and discussion of such schemes for future development will make them easier to carry out when the time arrives. Successful revolutions are those whose future policies have been most carefully worked out beforehand.

NOTE

It should be stated quite clearly that in the concluding paragraphs I am referring to museums in general, with no particular museum in mind. In many, a brave attempt is being made to break with the evil traditions of the last century.

The Site of Troy

by WINIFRED LAMB

THE name of Troy, more touched with glamour than any other name in history, brings to the archaeologist, the literary critic and the ordinary man, associations so widely different that they are in danger of becoming altogether divorced. In the mind of the archaeologist, it calls up a series of unsolved problems that produce a faint sensation of irritation and anxiety. Can that tantalizing group of objects from Schliemann's excavations, most of which have been arranged more according to style than to stratigraphy, be regarded as a safe guide to the development of prehistoric Asia Minor? Which of the stone battle axes come from Town I and which from Town II? What really happened between the second and the sixth settlement? How does the material fit in with that from other early settlements in Asia Minor and adjacent lands?

The literary critic too is worried when he has to deal with the topography of the Troad in relation to the events described in the Homeric poems. Knowing much more about the latter than the former, he fails to fix a standard as to how far they should correspond.

The ordinary man, happily unconcerned with questions like these, remembers only with a certain thrill that the drama of the Trojan war found a fitting counterpart in Schliemann's dramatic discovery of seven—or was it nine?—superposed cities. He makes a mental note to ask the next archaeologist he meets how many there really were.

Much apparent coincidence is subconscious tendency, and we may explain as we please the fact that within the last few years both popular interest and scientific attention have been directed to Troy by a series of controversies, discoveries, and publications which have no obvious connexion with each other.

The excavation of a new settlement closely akin to the early stages of Troy in an area mentioned by Homer as the southern boundary of Priam's kingdom has solved some of the problems which used to handicap the archaeologist, while merely underlining others. A short

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account of this excavation, which was begun two years ago at Thermi in Lesbos, will be given below.¹

Homeric scholars, meanwhile, have been disturbed by the revival of doubts long dormant as to the relation of Troy, Hissarlik and the camp of the Achaeans. Some six years ago, Dörpfeld, Brückner and others voiced the view that there was no room for the Achaean camp and the advances and retreats which the *Iliad* describes between Hissarlik and the Hellespont, and since then a series of treatises has been written to prove either that Hissarlik is not Troy or that the Greek camp was not on the Hellespont. The latest of these, by M. Vellay, has just been given to us under the title of *Les Nouveaux Aspects de la Question de Troie*.²

Meanwhile another class of experts, the philologists, have been disputing as to whether the TAROISA on a Hittite text can be identified as Troy, giving the Homeric capital the sanction of a genuine historical document. Unfortunately the element of uncertainty, which lowers the value of the material remains and distorts the geographical problem, has entered this field of enquiry as well.

Finally the appearance of Ludwig's *Schliemann*³ at this juncture is particularly opportune, since the curious personality of the great excavator (whose archaeological note books show him to be distinctly more scientific than his biographer allows) is one of the tests of the authenticity of Hissarlik and the significance of the finds.

Here we will concern ourselves with two groups of problems only, the geographical, as presented by M. Vellay, and the archaeological as affected by the excavations at Thermi.

It is a healthy sign when well established hypotheses are attacked ; all to the good, therefore, that M. Vellay and his predecessors should undertake this offensive in the Troad. M. Vellay's position may be summarized as follows :—

(1) That if the Greek ships were drawn up on the Hellespont, Hissarlik cannot be Troy, because there is not enough room for the camp and the manoeuvres ; and because the present course of the Skamander is not suitable for the Homeric narrative.

(2) That it is impossible both on literary and on topographical

¹ Conducted by W. Lamb under the auspices of the British School at Athens. See *J.H.S.*, XLIX, 223, 224 and L, 247-8 ; also *The Times*, 11 August 1931.

² Paris, Société d'Édition 'Les Belles Lettres', 1930, pp. 134.

³ Emil Ludwig, *Schliemann of Troy* ; Putnam, 1931, reviewed on p. 108.

THE SITE OF TROY

grounds for the ships to be drawn up anywhere else. This is directed towards a recent theory of Dr Dörpfeld and others that they may have been at Besika on the west coast.

(3) Therefore, that Troy must be sought elsewhere than at Hissarlik, and Hissarlik explained otherwise. M. Vellay, reviving the thesis of Herr Seyk, explains it as the burial ground of the Achaeans.

In discussing the relation of Hissarlik to the Hellespont, and calling our attention to the small distance between them, M. Vellay is dealing with a well-worn theme; his remote predecessors are Strabo and Demetrios of Skepsis, his immediate forerunners Dörpfeld, Brückner, Seyk and others.

These are divided into two parties, one of which denies that the camp was on the Hellespont, the other that Troy was at Hissarlik. As most of the arguments advanced have been answered—or anticipated—by Leaf in *Strabo and the Troad*, I cannot do better than quote Leaf's statement that, as long as we do not accept the excessive estimate of Agamemnon's army in the *Catalogue*, 'an adequate force for the siege, or rather the masking, of a fortress at Hissarlik could easily find place along the sea shore from the lagoon to the In-tepe with a depth of say 400 to 500 yards; its outer lines of defence would be a clear two miles from the town; and the way in which the battle is described as surging backwards and forwards over the whole of this space in a single day shows that the poet cannot have had a much greater distance in mind' (p. 182). To this I may add that should the course of the Skamander not tally with the Homeric evidence, a point which seems to me 'not proven', there are two more easterly courses wherein it may have had its bed. It is certainly odd that M. Vellay ignores Leaf's arguments so completely, and only refers to *Strabo and the Troad* in one footnote, when he is otherwise so well informed on the literature dealing with his problem.

With Dr Dörpfeld's latest theory, that the Greek camp should be placed at Besika on the west coast, M. Vellay deals effectively. This theory involves a wider interpretation of the term *Hellespont* than that commonly accepted, for, according to Homer, the Greek camp was on its shores. Dörpfeld and his party are, on their side, able to quote authorities for extending the Hellespont as far as Besika and Tenedos: while M. Vellay, on his, demonstrates that Homer himself appears to confine it to the Dardanelles. He also shows very clearly that a camp on the west does not tally with a number of situations described in the

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Iliad, even though it be a suitable distance from Troy and on the appropriate side of the Skamander.

This aspect of the controversy is unimportant compared with the archaeological tragedy that lies behind it. In the year 1924 Dörpfeld,

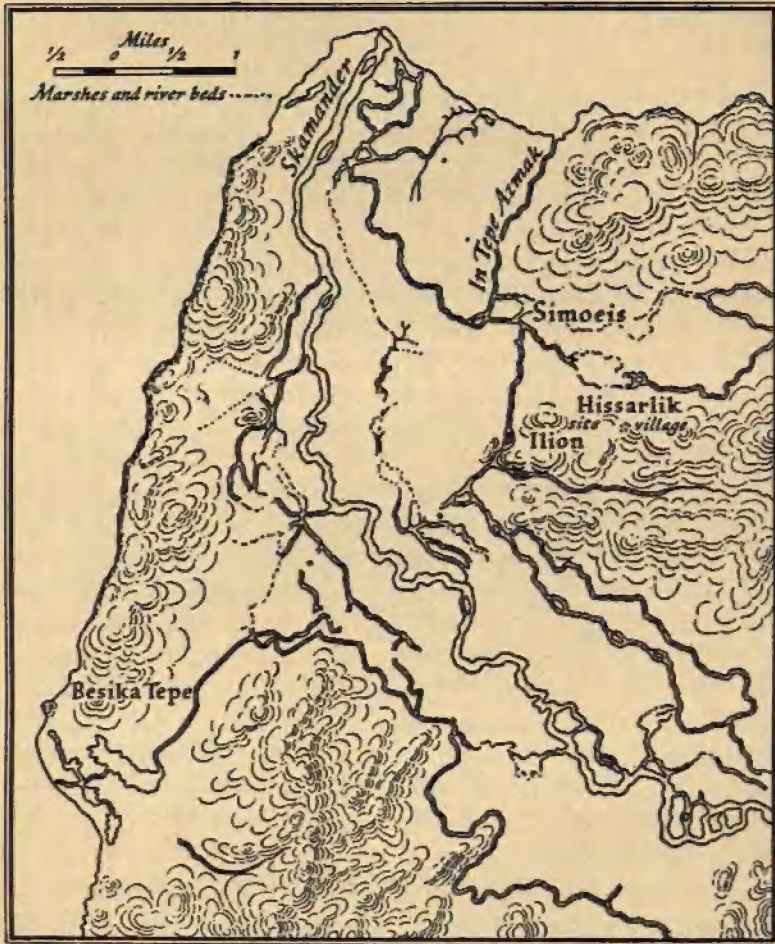


FIG. 1. The Troad (after Vellay)

Mey and Shede had the good fortune to obtain from the Turkish Government permission to conduct excavations near Besika. Here, with invaluable material for the prehistory of the Troad before them, two of the three scholars confined their attention to investigating the Homeric problem. To the third, Dr Mey, we are indebted for the

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only publication of the dig which has yet appeared, for a very careful account of the geology of the area combined with an excellent section of the trial trench, and for a commendably honest confession that the opportunities for studying the pottery were neglected. Should such an opportunity again present itself, attention should be directed to a type of pottery decorated with linear patterns in varnish paint, unparalleled so far in Asia Minor but offering tantalizing possibilities of contact elsewhere. It was one of several wares dug up on the mound by Schliemann; though the stratification is unknown, style, at least, suggests that our patterned sherds belong to the early Bronze Age or even to the Neolithic period, while a distinct Troy VI element indicates that Besika was inhabited in the Homeric period as well.⁴

Anyone who has read *La Question de Troie* thus far cannot fail to be impressed with the author's competence as a scholar, and with his logical and impressive arrangement of material. These qualities carry with them, inevitably perhaps, a tendency to press the Homeric evidence too far and to demand geographical accuracy from a poet whose very identity is obscure. When one considers that the *Iliad* has its roots in the folk-memory of the dark ages, and was frequently trimmed and pruned before it grew to its final form, we must expect a certain picturesque exaggeration of the attacking and defending forces and allow for a landscape that does not in every detail conform to that of the Troad. In short, M. Vellay looks at his problem too much from one point of view, and this becomes all too apparent when he passes on to advocate Herr Seyk's preposterous theory that Hissarlik is the burial ground of the Achaeans.

It is almost incredible that such a theory should have been put before a generation as familiar with the theories and probabilities of archaeology as the present. The objections are so obvious that one would not enumerate them here were it not for the still more incredible fact that more than one scholar has shown an inclination to become converted.

In the first place, then, the burnt matter in Hissarlik VI is closely associated with buildings and is at the same time less extensive than in the second settlement; nor could it be mistaken for the remains of a cremation burial save by people totally ignorant of what cremation burials look like. Contrast it with the pyres at Halos, and beware of

⁴ Publication of the material will appear in the *Prähistorische Zeitschrift* for 1933.

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saying, as M. Vellay does, that it has a close resemblance to certain Asiatic burial places (p. 112). No necropolis of the Troadic culture in Asia Minor has been dug except Yortan, where the bones of the dead are disposed in rows of pithoi; I am afraid he must be thinking of Bos Euyuk, which the first excavator believed to be the result of cremation, and which is now recognized as a village settlement frequently destroyed by fire. With considerable perspicacity, M. Vellay himself points out that one looks in vain for any likeness between his supposed tumulus of the Achaeans and anything discovered in mainland Greece.

In the second place, it would be very unusual, unparalleled indeed in the Aegean area, to have a cremation sandwiched between one series of settlements (nos. I-V) and another (nos. VII-IX).

The crowning objection, however, to the theory as expounded by M. Vellay is furnished by the diagram on p. 107. On the opposite page we are told that 'the fortifications of Hissarlik correspond exactly to the description given by Homer of the common burial ground'. (What quick work on the part of the Achaean troops!) The diagram shows an ideal construction drawn according to Homer's text, compared with the actual ruins of Hissarlik. We are certainly struck with the resemblance, till we realize that *it is the result of combining the walls of the sixth settlement with those of the second*. Moreover, is it not odd that a writer who accepts so easily the complete absence of his own Achaean fortification—('le mur qui traversait la plaine de l'est à l'ouest a naturellement disparu')—should object so strongly to the destruction of the north wall of Dörpfeld's Homeric city?

Hissarlik, then, is definitely a town site, and one of considerable importance. So much archaeology demonstrates, and any attempt to find another Troy must, as M. Vellay realizes, take it into consideration.

But what if there should, after all, be some other claimant lurking beneath an undug mound of the Troad? That is not impossible. It is sixty years since Schliemann's first excavations drew public attention to the ruins of Ilium, and nearly forty since Dörpfeld's brilliant analyses of architecture and strata concluded the campaign, yet the interval has added little to our knowledge of a district of Asia Minor, which, from every point of view, is of paramount importance. The most we can do, with the evidence at our disposal, is to try and estimate the position of Hissarlik by comparing it with other settlements belonging to the same culture; and even this comparison is more useful as a review of Western Anatolian archaeology than as a guide to Troy.

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In the second half of the third millennium, the culture which we may call Troadic spread over a large part of Asia Minor: its extent has been discussed by Myres⁵ and by Frankfort,⁶ who illustrates it by an excellent map. The places marked on this map are, for the most part, those which have produced isolated or unstratified specimens of the wares of 'Troy' I and II; a few are sites which have been partially excavated, or explored in relation to alien and later cultures.

In short, when we try to detach and examine settlement sites which may be compared with Hissarlik, we can find only four where anything is known of the strata: Bos Euyuk in Phrygia, the Toumba of Protesilaos on the Dardanelles, Hanai Tepe in the Troad, and Senirdje in the south. In all these our information depends, at most, on trial trenches and clearings, an unsatisfactory method of excavation; at least, on measurements taken by archaeologists in cuttings made for commercial purposes. All have two features in common: they show strata destroyed by fire, and they tell us nothing, or practically nothing, about houses and fortifications.

During the last three years, a new factor has appeared in the prehistory of the Troadic people. Lesbos, less than an hour's journey by sailing boat from the Anatolian shore, was colonized apparently no later than Hissarlik and about the same time as the Toumba of Protesilaos. At Thermi on the east coast the settlers built a small harbour town, which their descendants destroyed, remodelled and reconstructed till the remains of five towns lay one above the other.

One by one these towns have been uncovered, and the excavation, now approaching its last season, of their remains has been comprehensive enough to give us the whole plan of the uppermost town (no. v) and of about three-quarters of each preceding settlement, so that, for the first time, architectural material can be placed beside that of Troy.

But whereas in Troy I all the buildings we have are incomplete—the ends of walls uncovered by Schliemann's north-south trench—Thermi I and II, both contemporary with Troy I, produced a large number of houses. These are for the most part long and narrow, with a large room at the back entered through one or more small ante-chambers. Certain rooms seem to have been devoted to the storing

⁵ Myres, *J.R.A.I.*, 1903, p. 369 ff.

⁶ Frankfort, *Studies in Ancient Pottery*, II, 57 ff.

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of produce, for rows of pithos holes, some empty, some still containing fragments of store jars, run parallel with the walls. Hearths, common



FIG. 2. Western Anatolia, showing settlement-sites

at all periods, may occur in any part of the room. They are carefully constructed in layers: first comes a stone foundation, next a layer of pebbles, then a mosaic of potsherds, and finally a burnt earth surface.

The typical pottery of the period consists of soft black polished

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ware, and well baked red ware, both, of course, hand-made. Numerous small objects—copper pins, polished stone axes, bone tools—help to date the unstratified material from Troy I and II.

There follows a period when Hissarlik was deserted and debris accumulated on the site. To this interlude belong the third town of Thermi, the second village on the Toumba of Protesilaos, the cemetery of Yortan in Phrygia, and some of the pottery from Senirdje. At Thermi a new development occurs which throws light on the relation of Lesbos to mainland Greece. This is the introduction of *bothroi*, clay-lined pits below the floor or ground level, concerning the function of which there has been considerable controversy. Now these pits have long been known at Orchomenos, and have since been discovered in an early Helladic context at Gonia, Korakou, Zygouries and Eutresis on the one hand; in an early Macedonian stratum at Molyvopyrgo in Chalcidice on the other. Since both Greek and Macedonian examples are dated by the pottery with which they are associated to be later than Thermi III, we cannot help suspecting that the practice of making *bothroi* must have travelled from east to west.

Meanwhile the pottery from Thermi and the contemporary Anatolian sites develops steadily, showing a marked improvement in technique, new forms and more varied decoration. The whole period, in fact, was one of growth and activity in which Hissarlik did not participate. Can it have attracted the attention of some enemy through being already more powerful than its neighbours?

A more dramatic chapter in the prehistory of Anatolia opens with the founding of the second city of Hissarlik on a scale unparalleled so far in any contemporary town-site in the Aegean area. The magnificent fortifications, three times remodelled, and the stately approach to the centre of the town, are appropriate only to the metropolis of the Troadic culture; nor could any ordinary town have acquired the magnificent gold ornaments, the superb lapis and greenstone battle axes, and the other treasures that give an impression of commercial enterprise and wealth which find their only counterpart in Crete. It is, indeed, difficult to imagine that two such cities could exist simultaneously in one district.

We may assume, therefore, that from Hissarlik radiated the activity that carried the Troadic culture into Macedonia and influenced so unmistakably mainland Greece and the islands in the early Helladic period.

Thermi, as we should expect, reflected the prosperity of the Anatolian capital. The fourth town was built over the ruins of the

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third, on entirely new lines, oriented northeast and southwest instead of north and south. But the final achievement of this little community was Thermi v, contemporary, as far as we can judge, with the middle phase of Hissarlik II. Thermi v is characterized architecturally by its wider streets and by the appearance, in its very centre, of a single house recalling at first sight the well known *megaron* type with antae and three inner chambers. It fails, however, to take its place at the head of the series of *megara* because it is not detached from its neighbours and because its hearth is not central. We must, therefore, abstain from connecting it with the slightly later *megaron*-house of Troy II 3, which is the forerunner of the palaces of Tiryns and Mycenae.

The wall which protected Thermi v was broken by two gateways ; whether towns I-IV were also fortified we should learn during this year's excavation. The objects found within the various houses—vases, figurines, copper, stone and bone tools—link Thermi to the penultimate stage of Hissarlik II.

Now the final stage of Hissarlik II, characterized by the appearance of wheel-made pottery and by the *megaron*-house mentioned above, perished by fire at the beginning of the second millennium, while the villages of Bos Euyuk and Protesilaos seem to have shared its fate. The aggressors may, as Prof. Myres suggests, have been the Hittites. As for the Thermiotes, whose home shows no trace of violent conflagration, they may have anticipated the danger and migrated to another part of their island.

In any case, the Troadic peoples remained in a subordinate and devitalized condition through the confusing stages represented by the third to fifth settlements of Hissarlik ; nor can we again pick up the thread of our story till the foundation, at a date still under dispute, of that city which so many scholars believe to be Homeric Troy.

So we find ourselves back at the sixth city of Hissarlik where we originally started. Its walls, though different in style to those of the mainland palaces, are no less impressive ; its surviving buildings, though few, are distinctive, and no one who has read *Troja und Ilion*, pp. 150-175 could say 'de cette Troie VI il ne reste rien, sinon un fragment de muraille'. Its area, which seems to M. Vellay inadequate, is practically the same as that of Gonia and not much smaller than that of Phylakopi : it seems to conform to the standard of the times, though I must confess that there is very little material in the Aegean available for comparison owing to the scarcity of town-excavations.

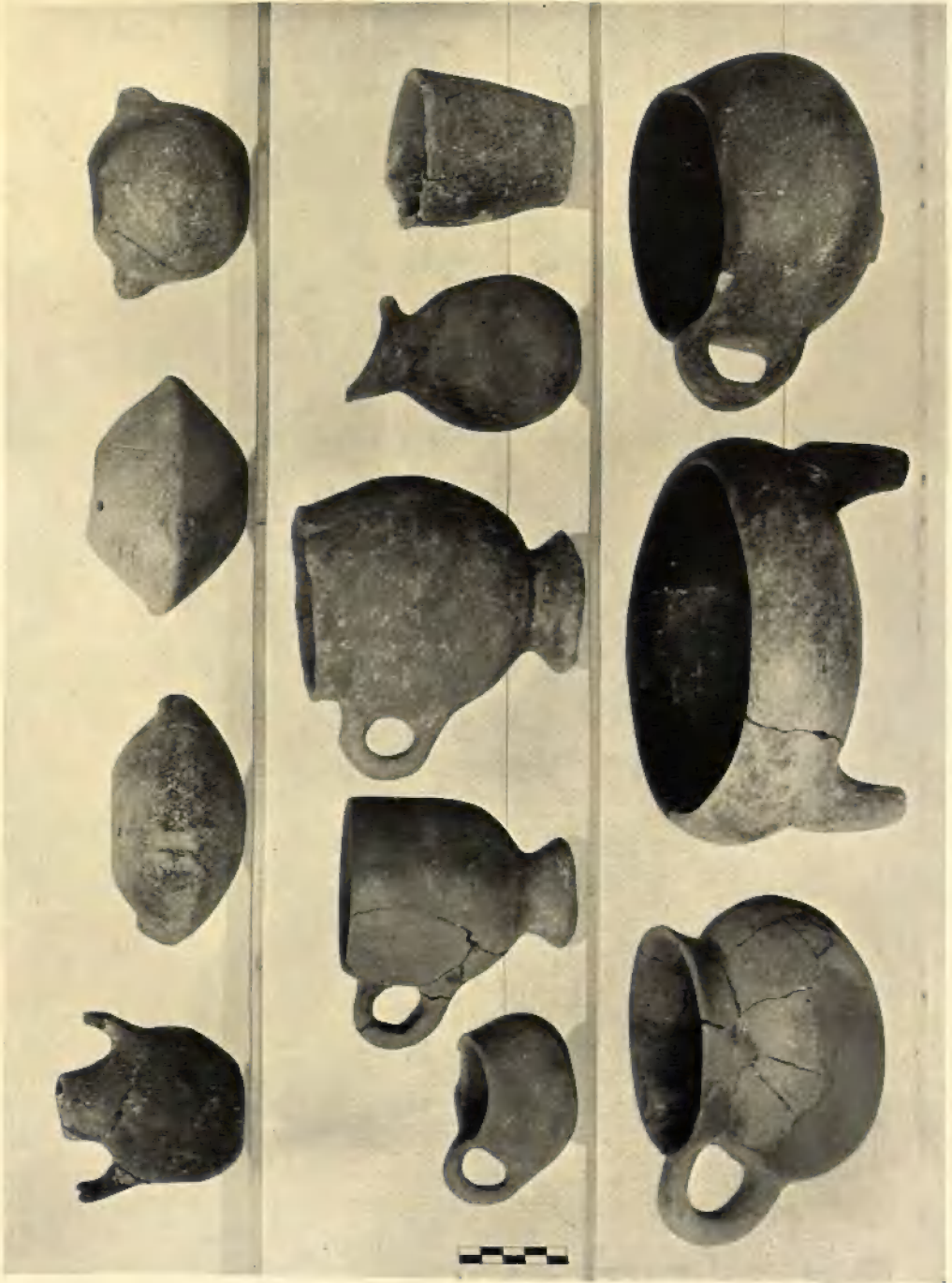
The finds from Hissarlik VI deserve special mention, for they

PLATE I



THERMI : PART OF TOWN V

PLATE II



THERMI : VASES FROM TOWNS IV AND V

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include, besides Mycenaean sherds, a peculiar type of grey pottery, sometimes plain, sometimes decorated with wavy lines or with attachments in the form of horns and animals' heads. This pottery is of all Troadic wares the most interesting: on the one hand, its connexion with Minyan ware, first demonstrated by Mr Forsdyke,⁷ is becoming more apparent each year; on the other, the possibility of its development into the Aeolic bucchero of the seventh and sixth centuries is indicated by recent finds from Methymna and Antissa in Lesbos. Its position in the history of ancient pottery would, I think, become quite clear if we had stratified material from the rest of Troad to put beside that from Hissarlik; unfortunately so little study has been made of Troadic archaeology at this period that we can predict nothing with certainty. Compare for a moment our knowledge, slight as it is, of the early Bronze Age in these lands with our almost complete ignorance of the period between 2000 and 1100 B.C. Hissarlik stands isolated in the general obscurity, and if it seems singularly defenceless before the attack of M. Vellay, it is after all the archaeologists who are to blame.

For, if there is any moral to be drawn from this mixed account of M. Vellay's book, the prehistory of northwestern Asia Minor, and recent discoveries in Lesbos, it is the need for further excavation. Thus only can we get a reliable solution of the problems of archaeology, ethnology and topography which form such a handicap to research. A comprehensive examination of the early remains in the Troad will show once for all if any parallel exists to the hitherto unique ruins excavated by Dörpfeld and Schliemann, and prove their right to bear the name of Troy. To Lesbos as well as to the Troad we may look for a sequence of wares covering the Hissarlik III-V periods, and for information about the development of the later grey wares; but traces of invasions, such as those which Prof. Myres postulates from the dolichocephalic skulls in the Third Settlement, and of oriented contacts, like that between Troy and the Hittites, must be sought in Asia Minor itself.

There is reason to hope that this will become possible sooner than any of us anticipated, and that the clue to what we seek will be placed in reliable hands. In the meantime, every sign of interest on our part, every small concession won, is a step in the right direction and increases the likelihood of ultimate achievement.

⁷ *J.H.S.*, xxxiv, p. 144.

Notes and News

NENNIUS'S CHRONOLOGICAL CHAPTER

Mr A. O. ANDERSON has sent us the following extract from an unpublished work of his on 'Materials for the History of North Britain in the Fifth Century':—

HISTORIA BRITTONUM, chapter 66 (translated from the text in MONUMENTA GERMANIAE HISTORICA, AUCTORES ANTIQUISSIMI, XIII, 209).

From the world's beginning to Constantinus and Rufus [A.D. 457], 5658 years are found (1).

Also from the two Gemini, Rufus and Rubelius [29], to the consulate of Stilicho [400], are 373 years (2).

Also from Stilicho to Valentinianus, son of Placida, and the kingdom of Guorthigirnus, 28 years (3).

And from the kingdom of Guorthigirnus to the quarrel of Guitolinus and Ambrosius are 12 years; which is *Guoloppum*, that is, *Catguoloph* (4). And Guorthigirnus held empire in Britain when Theodosius and Valentinianus were consuls (5); and in the fourth year of his reign the Saxons came to Britain, when Felix and Taurus were consuls [428], in the 400th year from the Incarnation of our Lord, Jesus Christ (6).

From the year in which the Saxons came to Britain and were received by Guorthigirnus, down to Decius and Valerianus, are 69 years (7).

NOTES

1. *reperiuntur*; in Victorius, *referuntur*. 5658 A.M. = A.D. 457. This calculation is taken from some copy of Victorius of Aquitaine's *Cursus Paschalis* (M.G.H., *Auctores*, IX, 682), which was published to take effect from A.D. 457.

2. This means that Stilicho was consul in 373 A.P. (Victorius) = A.D. 400, and is correct. The consulate of Rufus and Rubelius (in reality A.D. 29; but a year earlier, according to Victorius) was the year accepted by Victorius as the year of Christ's crucifixion. The calculation in the text was taken through Victorius from Prosper, and

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also from Sulpicius Severus's History, II, 27 (Patrologia Latina, book xx, 144).

3. From the consulate of Stilicho (A.D. 400) to the reign of Valentinianus and Placidia (A.D. 425) are 25, not 28, years. This is a calculation, made in connexion with the *Historia Brittonum* extracts from a *Liber S. Germani*, of the date of Germanus's visit to Britain; it is no doubt derived from Prosper, who places the visit in A.D. 429. As it stands, the calculation in the text would make Valentinianus and Vortigern begin to reign in 428; but that is probably intended for the year of the visit of Germanus; especially if the date given below of Vortigern's accession (425) was entered by the same writer. The Lives of Germanus say that Germanus (†448), on his second visit, left Britain after the death of Vortigern (†446×), and died in the reign of Valentinianus III (†455) and Placidia (†450): but it is not there stated that Vortigern reigned in Britain at the time of Germanus's first visit.

4. This is derived from some Welsh source, now lost. The word *Guoloppum* (erroneously *Guoloppuni*, in Harleian MS. 3859, fo. 189 verso) probably means 'at the villages of Wallop', on the Hampshire river Wallop; *Catguoloph*, 'the battle of Wallop'. See O. G. S. Crawford, in *ANTIQUITY*, v, 236-8.*

5. This statement that Vortigern began to reign in the consulate of Theodosius and Valentinianus (A.D. 425) is derived from the statement in chapter 29 of *Historia Brittonum* that the previous ruler in Britain, Maximus, was killed by those *consuls*; a statement erroneously derived from Prosper of Aquitaine, who says that Maximus was killed during the reign of the *emperors* Theodosius and Valentinianus (A.D. 379-391).

6. By 'the fourth year' of Vortigern, A.D. 428-9 is meant: Felix and Taurus were consuls in 428. For the 'Incarnation' we must certainly read 'Passion'; and Mr Anscombe says that most MSS. have '401st'. 401 A.P. (Victorius) = A.D. 428. But the writer of this chapter 66 seems to have placed both the arrival of the Saxons

*Since Mr Anderson's note was in print, I have discovered another Wallop in Shropshire. It is not unlikely that this may have been the original name of the river Rea, rising northeast of Montgomery and joining the Severn near Shrewsbury. Further research is required here to discover early forms and the precise attribution of the name.—O.G.S.C.

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and the visit of Germanus in the one year, A.D. 428. This is a deduction from the *Liber S. Germani*, and has no independent authority.

7. 69 years after 428 would bring us to A.D. 497; Decius was consul in A.D. 486. There is therefore some textual error, which would no doubt be explained if we found the source from which this calculation is derived.

According to De Rossi, Paulinus, the consul of 498, was a Decius (*Inscriptiones Christianae urbis Romae*, I, 493; cf. Mommsen, *Auctores*, XII, 495, s.v. Inportunus). De Rossi based this assertion upon the Letters of Cassiodorus. The more probable explanation of the text is that there is an error in the numbers of the calculation. For 'to Decius and Valerianus' we must read 'to Decius', with Gutschmid and Mommsen; and for 69, perhaps 59 years.

Chapter 66 of the *Historia Brittonum* is a collection of quotations and deductions drawn from known and unknown sources. It has no original value for the date of Vortigern's reign, or for the date of the arrival of the Saxons.

Immediately after the passage translated above, and before Nennius's 'Marvels of Britain', the 'Annales Cambriae' and Welsh pedigrees are entered in Harleian ms. 3859, fos. 193-5 verso.

RECENT DISCOVERIES IN SHETLAND





At the extreme southern end of Shetland, and on the western coastline of the low-lying isthmus that links the Ness or headland of Sumburgh to the mainland, there was discovered some 25 years ago a group of prehistoric ruins that centred round a broch, to which the bulk of them were secondary. A large part of the group was thoroughly explored at the time by the proprietor, the late Mr Bruce of Sumburgh, but progress was hampered by the existence of a ruined dwelling having no great antiquity, but over which Sir Walter Scott had cast a glamour, bestowing on it the fictitious name of Jarlshof. The various remains on the site having been handed over to the care of H.M. Office of Works, it was decided to make a further exploration. The supervision was entrusted to Mr A. O. Curle, F.S.A., and results of particular interest were obtained last summer in the limited period during which the excavation was carried on. At a distance of some 50 yards inland from the sea, and in rear of the secondary buildings above referred to, there was found a dwelling sunk as regards its floor level to a depth of 6 feet at shallowest below present surface, and some 5 feet below the ground

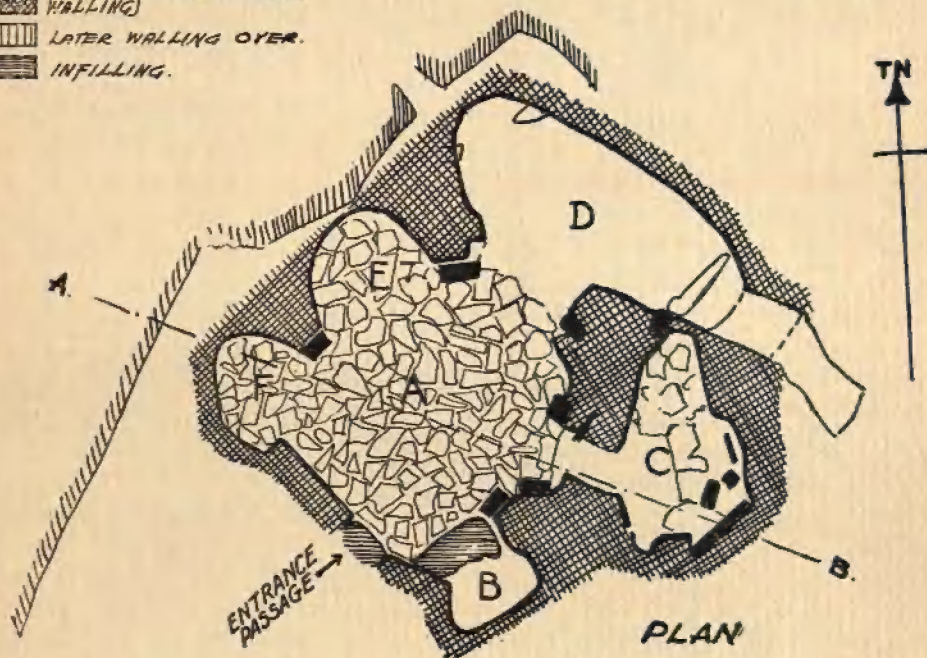
NOTES AND NEWS

JARLSHOF, SHETLAND
CHAMBER



SECTION A.B.

-  UPRIGHT
 -  WALLING
 -  LATER WALLING OVER.
 -  INFILLING.
- } SAME PERIOD.



PLAN

0 5 10 15 20 25 FT

AUG. 1901.

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level of its epoch, with walls formed of flat unhewn sea-worn boulders still standing to a height all round of from 3 to 4 feet. The exploration of the interior was complete up to the inner end of the entrance passage, and revealed a house measuring some 18 by 20 feet in length and breadth, consisting of a main chamber, or hall some 12 feet long, out of which there opened two large chambers and three cells of which the hall and two cells were carefully paved. One of the larger chambers had been used as a cook-house for in it were found the hearth, food refuse, bones of domestic animals, of birds and of fish, cereal grains, portions of saddle querns and rubbers. The bulk of the remains found disclosed a neolithic culture, rude stone axes, saws of slate, scrapers of white quartz, and adzes of bone; but the discovery of a number of fragments of clay moulds for casting bronze implements, among which pieces of moulds for a sword and a bronze socketed axe are recognizable, show that the period of the occupation is clearly of the Bronze Age. The significance of this discovery is marked by the fact that the record of bronze in Shetland is confined to one object, a spear-head. An interim account of the excavation has already been communicated to the Society of Antiquaries of Scotland, and it is hoped, if funds permit, that the work may be continued next summer.

THE PLETTENBERG SKULL

Dr L. H. DUDLEY BUXTON writes:—

'The recent discoveries of early man and of his artifacts in South Africa have excited considerable scientific interest, and recently we have had the opportunity of examining various skulls and a long series of beautiful photographs of existing racial types from that region in the Wellcome Historical Medical Museum in Wigmore street, London. Professor Drennan, who has done so much to further these studies, now reports a new skull from Plettenberg Bay, about 300 miles east of Cape Town. No data are at present available as to the age of this interesting specimen, and details of its exact character are lacking. It appears however to be remarkable for its extremely large size—a characteristic, it will be remembered, of the Boskop skull—and to be remarkably different from the Bushmen, who are notably small brained. Professor Drennan is of opinion that this large size is due to the retention of foetal characters in the adult, and that the skull represents a forerunner of the Bushman, and shows how man has arisen from a lower form. Without further details it is difficult to discuss critically Professor Drennan's most interesting theory. The position of the Bushmen is a

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problem of considerable difficulty. In some ways they are noticeably different from the other races of man ; presumably they originated in South Africa. Sir Arthur Keith has drawn attention to the affinities of the Boskop type—to which as far as one can gather from the scanty details this new skull belonged—with the Bushman-Hottentot type, but differing from them in having large brains. It is not improbable that we are actually dealing with a type that is akin to the ancestral type of man, although already specialized in the Bushman direction. The actual mechanism of evolution must be considered uncertain, and therefore the publication of Professor Drennan's detailed studies will be awaited with particular interest'.

THE ANTIQUITY OF MAN

Miss CAROLINE RYLEY writes :—It has always seemed to me that arguments for the antiquity of Man have been based too exclusively upon 'finds', and too little upon other, possibly equally cogent, considerations. The conclusion has been forced upon me that the dating based upon finds cannot possibly explain the equally well-established facts of distribution.

The first, and fundamental, hypothesis that I would like to put forward is, that most of the 'finds' of sub-Man himself are not, as has been implicitly assumed, *representative* of the period to which they belong, but *survivals* from an earlier time. Almost a corollary to this is the hypothesis that the locality in which they have been found was, so to speak, a 'back-water' region of their own time—this fact helping to explain the reason for their survival both in their lifetime, and, as fragmentary remains, to this day ; the more progressive regions having ousted them in their day, and swept away their remains in the more active life that followed. Colour is, incidentally, lent to these time-hypotheses by the 'marginal' character of the localities in which were discovered the remains of *Pithecanthropus* and *Sinanthropus* respectively. Such marginal regions might well allow an out-of-date form of life to survive while the rest of the world was (comparatively speaking) hustling forward ; and allow their remains to lie undisturbed until the changes due to lapse of time themselves would aid in the work of permanent preservation.

To regard *Pithecanthropus*, say, as a survival rather than as representative of his age would help to explain an otherwise inexplicable fact of the widespread distribution, during the Ice Ages, of a 'man'

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(whether sub-Man or *Homo sapiens*) intelligent enough for the Chellean and Acheulean cultures.

There would seem to be grounds of distribution and differentiation for the assumption that the work of the Chellean and Acheulean periods was that, not of sub-Man, but of *Homo sapiens* himself. . . . For *Homo sapiens*, at the close of the Ice Ages, is not only 'evolved', and in possession of a strictly limited field—he is widely distributed over areas which have been barricaded off from each other for the greater part, if not the whole, of the Ice Ages. Not only this, but he has been long enough in existence to have evolved physical types, or 'races', of which the fundamental characteristics are so deeply rooted as to be permanent—or at least to have survived until now. Upon what hypothesis can this world-wide distribution of *Homo sapiens* at the close of the Ice Ages—this deeply marked differentiation into races—be explained, if not by the assumption that, at the opening of the Ice Ages, *Homo sapiens* was not only already in existence, but already distributed over areas which, in the Ice Ages, became almost impassably separated from each other (thereby giving the conditions for differentiation) ?

As to *where* the emergence took place, and where was the centre of radiation—that is an entirely different matter. Judging from the facts of distribution, one would expect it to be fairly central as regards the 'Old World'—one whence a distribution and a growing differentiation could be carried on in Europe, Asia, and Africa, more or less 'pari passu'. Judging from established facts of later developments (especially those concerned with the emergence of civilization from a mere food-producing stage) one would expect the evolution from sub-Man to have taken place under conditions which were at the same time stimulating (or 'urgent') and encouraging—such conditions as may perhaps be roughly approximated to those of our present-day 'Warm Belt'.* Possibly the Chellean and Acheulean artifacts are those of *Homo sapiens* in situ (though not delimiting his distribution); forced southwards and sunwards by recurring Ice Ages, but returning in the warm interludes of which the so-called 'Post Glacial' may be one. In this later 'Warm Stage' he would, however, find his more congenial habitat somewhat further south than formerly—*i.e.*, in the Mediterranean and similar regions. Though adapting himself also to harder conditions in a colder clime, here would, and did (?), take place, his

* The later developments have been worked out in detail, but quite independently, by De Geer ('The subtropical belt of Old World Empires', see pp. 118–20, *post.*)—ED.

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evolution of the food-producing stage of culture, and of that more properly called civilizations.

The conditions of the 'Warm Belt' have always, as far as facts can be established, been conducive to 'progress' properly so called. To it belong, probably, the emergence of man from the 'savage' stage of a dependence upon wild 'nature'. To it belong, certainly, the first perfecting of beautiful handicrafts, the first shaping of efficient tools in metal, the first reliance on acknowledged law, the first architecture, art, literature; the first conception of spiritual religion. The great achievements of the Greeks and, later, of the Lombards, were those of a race with the physical and mental robustness of cooler climes plunged into the opportunities of a civilization evolved in warmer regions (now grown too warm for the maintenance of an effectively progressive physical type). The achievements of early modern 'Western' Europe are those of the same Northern races enkindled by (mental) contact with the South, through the Renaissance.

Possibly those conditions which have produced the most notable established facts of human progress, were those also of the emergence of humanity itself—of *Homo sapiens*.

However this may be—and wherever this emergence may finally be located—there is surely some solid ground, on the known facts of distribution and of differentiation, for the following hypotheses as regards the *time* of the emergence:—that the discovered remains of sub-Man are those of forlorn survivors of an otherwise already extinct type of *hominides*; and that humanity proper, *Homo sapiens*, had not only emerged, but was fairly widely distributed in the Northern Hemisphere before the oncoming of the Ice Ages.

NEOLITHIC POTTERY

It is only within living memory that British neolithic pottery was recognized, but our knowledge of it since then has increased rapidly. The following is a summary of a recent lecture by Professor Gordon Childe. It first appeared in *The Times* (11 Nov. 1931), and as here printed it has the Professor's approval as a correct summary.

Professor Childe re-stated the division of British 'Stone Age' pottery into two quite distinct groups—Windmill Hill and Peterborough wares. Dealing with the Windmill Hill class, he said that his collaborator, Mr Stuart Piggott, would show that despite local and temporal variations it preserved a striking uniformity from Sussex to the Orkneys, from the North Sea to the Atlantic coasts. The leathery character of

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the vases revealed them as belonging to a great family, extending all across Northern Gaul from the Rhine to Brittany and the Channel Isles, comprised under the general name Western. This family might be divided on the Continent into three principal groups, localized respectively in Belgium and the Rhine Valley, in the Seine basin and in central France, and in Brittany.

As a whole the British pottery showed no special affinity to any one of these, so that it might rank as an independent group that split off from the Western stem before the specialization of the rest. Exact agreements, however, linked a small sub-group of the Windmill Hill ware localized in Scotland and Northern Ireland, with a similarly restricted group in Brittany. These were the sole valid evidences for the theory which derived the Windmill Hill culture from Armorica and brought it to our shores along with long barrows and dolmens. That theory must, however, be rejected on other grounds and the agreements noted above explained by a reaction on Brittany from Britain. Windmill Hill culture either came from northeast Gaul across the Straits in pre-Megalithic times or direct from the Iberian peninsula with the chamber tomb complex, in which case the analogous cultures in Gaul would be parallel but independent offshoots of the same stem.

The affinities of Peterborough ware (which flourished principally in Eastern Britain) lay notoriously on the Baltic. A series of slides illustrated astonishing correspondence in curious patterns made with twisted threads and cords between British vessels and others from Denmark, Sweden, Finland, and Russia. The rise of this style could be traced only in the Ukraine, whence it was transmitted again in pre-dolmen times to Denmark, and eventually to Britain. This country was, accordingly, the meeting place of two cultural currents even at the beginning of the New Stone Age.

THE BEAKER INVASION OF BRITAIN

Miss MARGARET MITCHELL sends the following observations on the paper in our December number by Mr J. G. D. CLARK, whose reply is appended:—

Mr J. G. D. CLARK has adduced evidence from England to show that the beaker invasion of Britain was of a dual character—a fact already brought forward by Professor V. G. Childe in his *Bronze Age*. The evidence from Scotland leads to a similar conclusion. Distribution maps show that the penetration of the 'A+C' complex has been principally by seaboard or river valley routes from the North of

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England. Yet beaker settlement sites at Hedderwick, Gullane Bay, North Berwick and Tents Muir in Fife—all suitable landing places on the East coast south of the Firth of Tay—have yielded 'A+C' sherds which show that some at least of the invaders must have come by sea. This disagrees with Mr Clark's assertion that the northern beakers demonstrate a subsequent development rather than a point of arrival. On the other hand the distribution of the B beakers in Scotland is predominantly coastal and points to a sea-borne invasion probably independent of the 'A+C' penetration.

In treating of the different types of beaker represented in Britain, Mr Clark has omitted to mention one important form, designated by Abercromby BC. It is due to the crossing of a pure B tradition with the debased C variety. Now in Scotland BC and C beakers predominate on the eastern seaboard of Aberdeenshire. It would seem therefore as though the B complex arrived after the 'A+C' tradition was already established there and that the resultant intermingling produced the hybrid BC. This conclusion would bear out Mr Clark's suggestion that there was a chronological difference in the arrival of the two complexes.

On the West of Scotland the 'A+C' group is well represented and includes three very early examples from Closeburn (Dumfriesshire), Stoneykirk (Wigtownshire), and Kilmartin (Argyllshire). Their provenance points to a relationship with the northwest English beaker group rather than with the Northumberland area, and they must be accounted as early as, if not earlier than, the East Scottish examples south of the Firth of Forth. Pure B beakers are represented on the West coast by single finds as at Largs on the coast of Ayrshire, or in settlement sites such as the Island of Coll and Ardnamurchan Point. Dr Cyril Fox (*Arch. Camb.*, 1925) has made the tentative suggestion that some of the West of England and Welsh beakers may have come direct by sea, but later he abandons the idea in favour of an overland route from East Anglia for which there seems much less evidence. I would here reassert the theory of a sea-borne invasion from the southwest as providing the most rational explanation of the distribution of B beakers on the West Scottish seaboard, as well as for the more northerly 'A+C' group in Lewis, Uist and Skye.

REPLY by Mr CLARK:—

I must hasten to agree with your correspondent that the evidence from Scotland confirms that of England and Wales in demonstrating

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the dual character of the beaker invasion. My only reason for omitting the Scottish evidence was that the dearth of objects associated with beaker burials and the restriction of human settlement to a fraction of the country, owing to the infertile and inhospitable nature of the greater part of its area, combined to render unsuitable the application of my method of research.

It is necessary, however, to correct a certain misapprehension, for I appear to have been misread into asserting the northern beakers in general to 'demonstrate a subsequent development rather than a point of arrival'. If my remarks are re-read it will be appreciated that I refer only to the 'A+C' group (ANTIQUITY, 1931, p. 418 top). I agree entirely that the 'B' beakers of Scotland may well be explained as direct arrivals from the Continent, but, returning to the 'A+C' group, I do not see anywhere in my paper any claim that they arrived exclusively, whether by land or sea, from northern England. At the same time I feel myself to be on safe ground, when I say that, for the most part, 'they represent a subsequent development'. If I have made this 'assertion', I fear that I am unrepentant and will continue to make it until faced with evidence of sufficient weight to render my position untenable. It may not be out of place here to point out that this was the view of Lord Abercromby himself, so that my 'assertion' may be said to dwindle into a meek statement of the orthodox view. It is incumbent on your correspondent to find the destructive arguments: I am content to rest upon the very solid evidence adduced by Abercromby. Many beakers have been found since the publication of his great work, but there is no reason to suppose that they would materially affect the conclusion to be drawn from the following condensation of his table:—

Beaker types			'A'	'AC'	'C'
South of Humber	97	0	2
Rest of England	11	37	76
Scotland	3	27	130

I submit that the only possible deduction to be drawn from this table is that type 'C' beaker is a development from type 'A' via 'AC', the geographically intermediate distribution of 'AC' being most significant.

Your correspondent seems to be worried over my omission of any specific reference to type 'BC'. As it was embraced in my remarks concerning hybrids and is furthermore numerically unimportant in the area with which my paper dealt, I saw no immediate necessity for

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doing so. Since it is mentioned, however, we may enlarge upon its significance. In bringing forward further very welcome evidence in support of my suggestion of a chronological difference between the 'B' and 'A+C' complexes, your correspondent argues for the view, in which I concur, that the 'B' complex is the earlier of the two, producing by admixture the 'BC' group of which we are speaking. It will occur to the reader that, in view of the fact that Abercromby records no fewer than 126 'BC' beakers and not a single 'AB' hybrid from Scotland, we may further infer that the 'C' type of beaker is subsequent in time to the 'A' beaker.

On the basis of these two lines of evidence I re-assert my belief that the 'C' beakers of Scotland, representing over 97 per cent. of the 'A+C' complex of that country (according to Abercromby), are a subsequent development of British 'A' beakers, and not a fresh arrival from the Continent. I must thank your correspondent once more for the interest shown in a problem which has been on my mind for some time, and especially for drawing attention to the matter of the 'B' hybrids.

STURGEON IN ANGLO-SAXON TIMES

Dr J. TRAVIS JENKINS, Superintendent of the Lancashire and Western Sea Fisheries Joint Committee, writes in reference to the note on 'A Saxon Fish-pond near Oxford' published in *ANTIQUITY* for December 1930, pp. 480-3:—

'Although the sturgeon is now a very rare fish in British waters there is evidence that it was abundant in Anglo-Saxon times. In addition to the 'Styrian Pol' of the *Cartularium Saxonicum* there are two references to 'Stirigan Pole' in the fourth volume of Kemble's *Codex Diplomaticus*. This is the first reference I have ever seen to the keeping of sturgeon in ponds, as it is undoubtedly the sturgeon that is referred to. *Porcopiscis* is the dolphin (a mammal), *rombus* is the turbot, but *cragacus* cannot be associated with any known fish. I should think the association of these names with Styria was due to ignorance.

'An extensive search reveals no records of the keeping of sturgeon in ancient or modern times in storage ponds, and I should be grateful for any references to this subject'.

Recent Events

The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.

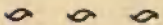
At Egtved in Jutland an oak coffin has been found containing the body of a girl of the Bronze Age whose face and clothes were in a good state of preservation. This was due to the preservative property of the bark. She had short hair bound with a ribbon; she wore a brown woollen jacket with long sleeves and no buttons, and a very short skirt kept in position by a fringed belt. Her ornaments consisted of a pair of ear-rings, a necklace, two bracelets and a brooch. At her side was a beer-mug in which traces of grain and honey have been detected. She was in fact a very modern young person (*Rev. Arch.*, xxxiii, 333-4).



We are indebted to one of our readers for the information that at Roknia (near Constantine in Algeria), where a very extensive 'dolmen-field' has been known for a long time to exist, there are also to be found *rock-cut tombs*. It is very much to be desired that plans of these should be made—as also of the dolmens; and by plans we mean careful surveys like those published in *Archaeologia*, 1927, LXXVI, 121-60, by Mr W. J. Hemp, of the Majorcan rock-cut tombs. Here is an opportunity for post-graduate work.



The Petrie Medal was awarded again this year by the University of London. The award is made once every three years for the publication of outstanding archaeological work in the field. The previous holder was Sir Aurel Stein whose work in Central Asia is famous. On Foundation Day, 28 November, the medal was handed by the Vice-Chancellor to Sir Arthur Evans, of Cretan fame.



Dr Wheeler's courses in British archaeology, held in connexion with the University of London, have been continued during the winter. They include a visit to the Wiltshire downs at Whitsuntide.

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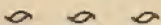
In 1913 Dr Hans Reck discovered a skeleton at Oldoway in Kenya. The site has now been revisited by Dr Reck in company with Dr Leakey, and further important evidence of the age of the deposit in which it was found has been discovered. The facts are however too complicated to follow without the plans and sections which will doubtless accompany the detailed report (*Nature*, 12 December 1931).



In the Pipe Rolls of the 12th century frequent mention is made of the Forest of Halla Haroldi, or Harold's Hall. Up to the present nothing has been known of this forest and hall beyond the fact that they were in the Hundred of East Meon in Hampshire. They are now identified by Mr Herbert Chitty from an unpublished document at Winchester College. In this, Harold's Hall occurs among the bound-marks of the Manor of Huntbourne 5 miles SE of Bishop's Waltham. The forest must therefore have been that later known as Bere. Mr Arnold claims to have located the ruins of the hall near Clay's Copse. The interest of the discovery lies in the fact that the Harold in question can only have been the famous Harold, the last Saxon king of England.



On the small inset plan of Bath on Speed's Map of Somerset (1610) is marked 'the Boate-stall'; and there is a picture of what seems to be a ferry. A man is crossing the Avon on a raft or punt, holding with both hands an overhead rope swung across the river. On an old plan of Bath of 1572 Boat-stall lane leads down to this ferry. At the mouth of the Parret Speed's map also marks 'Botestall poynt'. One wonders whether 'Boat-stall' meant simply (as it should) a place for boats, a wharf; or whether it had come to have some more special connotation.



A hut-site of the Saxon period has been found in a gravel-pit beside the Foss Way near Bourton-on-the-Water, Glos. It has been excavated by Mr G. C. Dunning and Miss Donovan, who took part in the excavation of Salmonsbury last summer. It is gratifying to know that, as a result of the interest aroused locally by these excavations, many other antiquities, which otherwise would have been overlooked, have been brought to notice. (*Gloucestershire Echo*, 18 December 1931; *Cheltenham Chronicle*, 19 December 1931, illustrations of pit after clearance; *The Times*, 24 December, p. 9).

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There is no end to the archaeological richness of English soil. We were accustomed to the discovery of ancient Greek coins and statues, of Etruscan urns and such like. But it is nearing the limit when a modern Turkish inscribed tomb-stone is found buried at Kirkley near Lowestoft! (*Eastern Daily Press*, 14 November). The facts are however quite certain, and capable of a relatively simple explanation (*ibid.* 17 November).



The International Congress of Archaeology will be held in London August 1 to 6 next, after which there will be excursions to sites of interest in the provinces. The headquarters of the Secretariat is at Burlington House (Society of Antiquaries).



A small hoard of gold objects of the Bronze Age has been found at Towednack near St. Ives, Cornwall. To judge from the illustrations they consist of two torcs and six rings. The torcs have recurved hooks and appear (so far as one can judge) to be formed of a solid bar of twisted metal, not of the ribbon type. (*The Times*, 2 January 1932, p. 6; *Western Morning News*, 30 December 1931; *Birmingham Post*, 31 December 1931).



Mr J. BUTTER, of Deventer, Holland, has kindly sent us the following note, together with specimens of the grain itself :—

‘ At a depth of one foot in a Celtic field near Emst I found, on the land of the owner Mr G. van ’t Laar an extinct variety of *Triticum vulgare* (Common wheat). The determination of the species is by Dr Florschütz of Velp. This was a deductive discovery. Since Celtic fields existed there I promised a guild for the first grain found there ; and it was found in January 1928. I excavated there and gave the results to Dr Florschütz’. The fields were described in ANTIQUITY, 1928, II, 85.

The specimens of grain were sent, as likely to be of interest, to a British expert who replied as follows :—‘ It has been the practice for some time past to try and identify prehistoric wheats from specimens of grain alone, but this procedure ignores the fact that carbonization itself induces changes in shape. It has been found, by artificially carbonizing grains, that the changes are normally big enough to make it difficult, and often impossible, to identify the species. There is

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therefore little hope of identifying wheat species unless we can get more than the grain'. With regard to the present specimens he concludes that most of it is wheat of some kind, though amongst it there *may* be a little barley.

This is an unfortunate position for archaeologists, since the more perishable parts of the ear are naturally the ones least often preserved; and it may be no fault of their's that they cannot give the expert what he demands. Every care should however be taken to supply him with, in addition to the grains themselves, specimens of the adjacent soil which may contain the desired fragments. We mention this for the guidance of future excavators.



An article by Professor G. Elliot Smith in *The Times*, 11 December (p. 15) summarizes the very interesting discovery made by Mr W. C. Pei, the geologist in charge of the excavations at Chou Kou Tien, of stone implements in association with the remains found there of Peking Man, and the evidence adduced by Professor Davidson Black that the use of fire was known even at that stage in the history of man. The first notice in *The Times* of these discoveries appeared on 4 November, and this was followed the next day by a letter from Professor Elliot Smith pointing out their significance.



The results of the excavations at Herculaneum during the past four years are reported by Professor Maiuri in *The Times* of 6 November (p. 15).



A summary of the excavations at Paestum is given in *The Times* of 12 January (p. 11), with an outline of a report prepared by Senator Giuseppe de Lorenzo, who does not agree that the ancient city was destroyed by floods, but rather favours a theory that its disappearance was due to a geological movement, which buried it, and that a reverse movement restored the ruins to sight.



Some remarkable funerary remains have been found by the Archaeological Survey of Egypt in Nubia, south of the Great Temple at Abusimbel, where tumuli of late Roman period built by an Ethiopian people have been opened. In one of these, skeletons of animals, their trappings of silver, harness set with precious stones, and other remains, point to the burial of an important personage. (*The Times*, 11 January).

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Chysauster, a stone-built Celtic village in the parish of Gulval, some three miles from Penzance, is described in a special article in *The Times*, 23 January (p. 11). It was a tin-mining settlement inhabited as early as the 1st century B.C., and continued in occupation for 200–300 years after that time.

Dr R. E. Mortimer Wheeler has contributed a special article to *The Times* (6 January, p. 11) reviewing the discoveries made during excavations on the site of Verulamium.

A very useful addition to our knowledge of Persian history has been made by Mr A. W. Davis, British Vice-Consul at Resht, Persia, who has read and copied the Old Persian superscriptions on the sepulchres of the Achaemenid kings near Persepolis. These are cut above the figures representing the 30 nations subject to the kings of Persia, and supporting a platform on which the kings are shown worshipping before the sacred fire. The particular inscriptions read by Mr Davis are those on the South Tomb, which is ascribed to Artaxerxes II, 404–358 B.C. As Mr C. J. Gadd says in his letter to *The Times* (7 January, p. 6) communicating the discovery, Mr Davis is to be congratulated on recording a feature which has hitherto escaped attention.

The third expedition conducted by Professor John Garstang to excavate the site of ancient Jerusalem is receiving support from the Museum of the Louvre as well as from English sources. He proposes to continue work on the sites of the Royal Palace and the Necropolis begun last season. (*The Times*, 4 December, p. 13).

Sassanian painting and sculptures have been found at Hira, a Neo-Persian site which is being excavated by permission of the Oxford-Field Museum Expedition under the direction of Mr Gerald Reitlinger and Mr Talbot Rice. We hope to have detailed particulars sent to us for publication.

We regret that an error was made in the notes on recent articles in the *Illustrated London News* printed in the December number of ANTIQUITY, p. 501. The chryselephantine figurine there mentioned was acquired for the Royal Ontario Museum of Archaeology at Toronto from a dealer in England.

Some Recent Articles

This list is not exhaustive but may be found convenient as a record of papers on subjects which are within the scope of ANTIQUITY. Books are occasionally included.

THE TEMPLE OF THE WARRIORS, by Earl H. Morris. *Charles Scribner's Sons*, 1931. pp. 251. 21s.

An account of the excavation and restoration of a Toltec-period temple at Chichen-Itza. The work was carried out by the Carnegie Institution of Washington and Mr Morris's volume, very handsomely illustrated, is a record of such achievement as is too seldom attempted in archaeology. It is unfortunate that the author, apparently in an attempt to popularize the subject, should write in a style which vacillates between the levels of a banal journalese and the murky deeps of a hilarious illiteracy. J. L. MITCHELL.

OCEANIA : a journal devoted to the study of the native Peoples of Australia, New Guinea, and the Islands of the Pacific Ocean. *Melbourne : published for the Australian National Research Council by Macmillan & Co., 1930-1931. Nos. 1-4, 7s 6d each.*

The aims of this new periodical are set forth in its sub-title. Its editor, Dr A. R. Radcliffe-Brown, defines the word 'study' from the Journal's view-point as that which is 'properly carried out only by scientists who have been specially trained for the purpose'. Neither his own training nor erudition can be questioned in a lengthy paper, 'The Social Organization of Australian Tribes', which runs the gamut of the first four numbers and dwarfs other contributions in both subject-matter and scholarship. Miss Camilla H. Wedgwood contributes various papers on the secret societies, wars, and other of the less hygienic activities of Melanesia, and is apt to assume, very unwarrantably, underlying rational purpose in savage ritual—while decrying rational thinking in primitive man. Mr A. P. Elkin, in no. 3, has an interesting paper on the 'Rock Paintings of North-West Australia' and neatly burkes the question of their origin, thus avoiding conscription in the ranks of Dr W. J. Perry's adherents. Dr Raymond Firth deals with 'Totemism in Polynesia' uninspiredly but conscientiously. All in all, a very welcome periodical. Some amelioration of the paucity of its illustrations would add considerably to its value. J.L.M.

EL TOKI MAGICO, por J. Imbelloni. (*Anales de la Sociedad Científica de Santa Fe*). *El Establecimiento Gráfico Tomás Palumbo, Buenos Aires*, 1931. pp. 24. Price not stated.

Sr. Imbelloni, pursuing his researches into native American affinities with Polynesia, compares, detail for detail, the words and formulae of the Araucanian

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and Maori 'ceremonials of enchantment' in connexion with the use of the *Toki*, or stone axe. A sketchy, but cautious and scholarly, treatment of a cultural item confirming the obtrusive influence of Oceania in pre-Columbian America. J.L.M.

ON THE DIFFUSION IN AMERICA OF PATU ONEWA, OKEWA, PATU PARAOA, MITI, AND OTHER RELATIVES OF THE MERE FAMILY, by J. Imbelloni. (*Journal of the Polynesian Society*, Dec. 1930). pp. 34. Price not stated.

Details the various discoveries, in North and South America, of examples of the shaped greenstone club or sword which went in Polynesia by the general name of *mere*. Photographs of the various weapons—including an ancient Mexican specimen apparently decorated by a Maya craftsman—are given, the antiquity of the Mexican *mere* by itself disposing of the contention that these weapons were introduced into America by Captain Cook. J.L.M.

WIRA KOCHA. *Revista Peruana de Estudios Antropologicos*. Editor: Julio C. Tello. Vol. I, no. 1. Enero-Marzo, 1931. Lima, Peru. Subscripción Anual en el extranjero: \$5; numero suelto: \$2.

Sr. Tello's new review is one of both promise and achievement. The editorial note sets forth the policy of the journal as an impartial study from all angles of the ancient Peruvians and their habitat. Among other notable contributions is a paper on the crops and food-stuffs of the pre-Spanish states, by M. T. Mejia Xesspe, and one on the use of the martinet bird in the decorative art of Nasca, by Eugenio Yanowleff. The number is satisfactorily illustrated with photographs and drawings. J.L.M.

Die Langmauer bei Trier und ihr Bezirk—eine Kaiser domäne, by Josef Steinhausen. *Trierer Zeitschrift*, vol. VI, 1931, heft. 2-3, 41-79, 7 plates, sketch-map.

Tardenoisien in der Krim, Neolithikum im Wolgagebiet, by Boris Schukov (Moscow). *Bericht über die hundert-jahr Feier*, 21-25 April 1929 (de Gruyter, 1930), 281-90.

An interesting account of carefully conducted excavations. Records the discovery of pottery associated with Tardenoisian culture in the Crimea which, if contemporary with the western phase, becomes the oldest ceramic of Europe.

Der Goldberg bei Nördlingen und die moderne siedlungsarchäologie, by Dr Bersu. *Ib. id.* 313-18.

A summary of the results of excavation in this important hill-fort which was occupied from neolithic times onwards.

Die Ergebnisse der Warfengrabung, by A. E. van Giffen. *Ib. id.* 322-38.

A useful summary of what is known about these important but little-known Dutch marsh-dwellings.

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The Rise and Progress of Classical Archaeology, with special reference to the University of Cambridge; an inaugural lecture by A. B. Cook, Laurence Professor of Classical Archaeology. *Cambridge University Press*, 1931. pp. 61. 2s.

A useful but necessarily brief outline. The special reference compels the exclusion of certain pioneers not associated with Cambridge (such as Horsley), but allows our old friend Stukeley to appear. We most heartily endorse the plea for the foundation of a Readership of British Archaeology, but should go further and demand a Professorship. British archaeology is the creation of the amateur; but it is now firmly established as a profession, and is badly in need of recruits. There is still, however, no adequately endowed and equipped university school of training; and vacancies in the profession have to be filled from outside sources.

Zur Megalithkultur Nordwestdeutschlands, by Ernst Sprockhoff (Römisch-Germanisch Zentralmuseum, Mainz). Sonderabdruck aus *Nachrichten aus Niedersachsens Urgeschichte* no. 4; 1930 (August Lax, Verlagshandlung, Hildesheim). pp. 55, plans, sketch maps and schedule.

Die Sprache der Festlandkelten, by Leo Weisgerber. *Deutsche Arch. Inst.* (Röm.-Germ. Komm.) 20th Report, 1931, 147-226.

Not the least valuable part of this article is the glossary of old Celtic words on pp. 191-214, which includes words such as briga, -duro, onna, Tamesa, vindo-, of interest to British students. There is a list of references of 272 items.

Ausgrabungen und neue Funde in Frankreich aus der Zeit von 1915 bis 1930 (Paläolithikum bis Römerzeit), by Raymond Lantier. *Deutsche Arch. Inst.* (Röm.-Germ. Komm.) 20th Report, 1931, pp. 77-146.

This is a valuable summary by a specialist of the more important discoveries made in France since 1915. Such occasional 'stock-taking' is becoming necessary in every branch of rapidly advancing knowledge; and M. Lantier's monograph (which from its nature is naturally incapable of further condensation here) will be found indispensable by all prehistorians and Roman students.

Unser Wissen von den alten Kulturpflanzen Mittel-Europas, by Fritz Netolitzky. 20th Report (1931) of the Römisch-Germanische Kommission of the German Archaeological Institute. (Joseph Baer, Frankfurt am Main), pp. 14-76.

A very valuable monograph, illustrated by two sketch-maps (ancient distribution of millet and dispersal-routes). It is quite impossible to do justice to it in a few lines, but it will be found indispensable by serious students. The last five pages contain a list of references arranged alphabetically under authors.

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New views on the relations of the Aegean and the North Balkans, by V. Gordon Childe. *Journ. Hellenic Studies*, L, part 2, 1930, 255-62.

The Religion of ancient Palestine in the light of Archaeology, by Stanley A. Cook (British Academy, Schweich Lectures 1925). Milford, 1930.

Etudes de Stratigraphie paléolithique dans le nord de la France, la Belgique et l'Angleterre, by the Abbé Breuil and L. Koslowski. *L'Anthropologie* XLI, 1931, 449-88.

We postpone fuller consideration of the above until publication is complete. The first part deals with the valley of the Somme only.

Les Hittites, by G. Contenau. *Ib. id.* 489-99.

Le progrès de la paléontologie humaine en Allemagne, by R. Vaufrey. *Ib. id.* 517-51.

A valuable summary of recent work, well illustrated.

Kritische untersuchungen über *Australopithecus africanus* Dart, by W. Abel. *Morphologisches Jahrbuch* 1931, LXV, 539-640. Reviewed in *L'Anthropologie*, XLI, 562-4.

Considerations sur certaines formes caractérisant l'âge du bronze de l'Europe sud-orientale, par Catherine Dunareanu-Vulpe. *Paris, Gamber*, 1930. pp. 60 and 3 plates.

L'âge du fer dans les régions thraces de la péninsule balkanique, par Rader Vulpe. *Paris, Gamber*, 1930. pp. 178 and 6 plates, map.

The Maltese rock-cut tombs of a late pre-Christian type, by Sir T. Zammit. *Bull. of the Malta Museum*, November 1931, I, no. 3, 101-31.

Les souterrains-refuges de Bas-Quercy, by R. Pouch. *Bull. Soc. préh. franç.*, November 1931, XXVIII, 465-69.

Faucilles préhistoriques de Ras Shamra, by G. Chenet. *Ib. id.* 469-75.

Un Glozel espagnol: les falsifications d'objets préhistoriques à Totana, by Juan Cuadrado Ruiz and A. Vayson de Pradenne. *Ib. id.* September 1931, XXVIII, 371-89.

M. Vayson de Pradenne is carrying on the good work of casting light upon the dark places of archaeology, and banishing the evil spirits that haunt them.

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Handbook and Guide to the Aegean and Hittite Collections on exhibition in the public museums, Liverpool, 1931. 3d.

The publication of guides to museums is always to be commended, and this one will be found useful by all visitors. Two minor blemishes should be corrected in the next edition: the typography (especially the title-page), and the book-list, which should give author's initials, the publisher, price and date. Those who may think initials a pedantic demand should turn up, say, Burn, Hall and Evans in the Bodleian catalogue. It might be a good thing also in the next edition to give more prominence to the writer's name which at present is concealed in the preface. The idea of publishing illustrations of the objects in their natural setting is an admirable one: and we are duly thankful for the pleasant little map (though the coast-line of Greece leaves something to be desired).

L'Afrique préhistorique, by the Abbé Breuil. *Cahiers d'Art*, 1930, v, 449-500.

Reviewed in *L'Anthropologie*, xli, 582-4. 'In this important work our knowledge of prehistoric Africa has been for the first time brought together and classified both geographically and (so far as possible) chronologically'.

Staré Osídlenie Slovenska [Ancient Slovak settlements], by S. Janský. *Sborník Muzeálnej slovenskej spoločnosti* (Turčiansky Sv. Martin), 1930, xxiv, pp. 67 and 17 plates; summary in French.

Reviewed in *L'Anthropologie*, xli, 590-1. 'The interest of this monograph, as of the author's preceding one on Slovak *oppida* (see *L'Anthropologie*, xli, 177), is greatly increased by the abundant topographical maps, with physical features shown, by which it is illustrated'.

Neolityczny grob skrzynkowy ze Skoczki, w pow. szubinskim [A neolithic cist-burial (tombeau à caisse), district of Szubin]. Publication d'hommage à W. Demetrykiewicz, Poznań [Posen] 1930. Summary in French.

Reviewed in *L'Anthropologie*, xli, 591, where it is claimed as evidence of megalithic civilization in Eastern Poland.

Lewis Henry Morgan: social evolutionist, by B. J. Stern. *Univ. of Chicago Press*, 1931, pp. 322.

Reviewed in *L'Anthropologie*, xli, 629-31. Contains a bibliography of Morgan's published and unpublished works.

Das Alter des frühgeschichtlichen Gräberfeldes von Ur, by Viktor Christian (Wien) and Ernst F. Weidner (Berlin). *Archiv für Orientforschung*, 1931, vii, 100-12.

A controversial article arguing on behalf of a later date than that assigned by Mr C. L. Woolley.

Reviews

SKARA BRAE: a Pictish Village in Orkney. By V. GORDON CHILDE, B. LITT. With chapters by PROF. T. H. BRYCE, F.R.S., and PROF. D. M. S. WATSON, F.R.S. *Kegan Paul, Trench, Trubner & Co.*, 1931. pp. xiii, 208, with 60 plates and 24 figs., plans and sections. 31s 6d.

By reason partly of its subject and partly of its own intrinsic value, this superbly illustrated volume is destined to figure as a standard work in British archaeology. In the Skara Brae settlement, dating in the author's opinion from about 500 B.C., are preserved the remains of at least 10 stone huts, the corbelled walls of which in some parts still stand to a height of about 10 feet. Since nowhere else in these islands is there known to exist a group of prehistoric dwellings in anything like the same state of preservation, their importance is obvious. And in comparison with its merits the defects of the book are so relatively unimportant that they may be dismissed very briefly. The most serious is a certain lack of lucidity—partly in expression and partly in classification. The distinction between 'structural periods' and 'occupation periods' is not kept sufficiently clear. It ought, of course, in justice, to be added that the buildings are so extraordinarily complex in levels and shape and grouping that a perfectly clear and simple description is well nigh impossible. Prof. Childe is aided however by what he justly designates the 'magnificent plans' prepared by Mr Houston of H. M. Office of Works (though it should be noted that at least one wall described in the text as 'd' is not found so designated on the plan). The spelling also of some of the proper names is occasionally at fault, and with reference to the so-called 'Lansbury Gallery' one may slightly modify the phrase to say 'que diable allait-il faire dans cette *galerie*'?

Taken as a whole, however, the volume is a monument of rare archaeological skill and insight. On the earlier part, which is devoted to a description of the site and the chronological sequence of the various structures, there is little need for comment; the facts are set out carefully, if not always quite perspicuously. But in the later chapters, which deal with the innumerable relics of stone and bone (no metal relics occur)—their nature and relationships—and the contingent dating of the settlement, it becomes ever more and more clear how fortunate was the choice when Prof. Childe was selected to superintend the exploration of this site. For the problems emerging are so unusually puzzling, yet of such general archaeological significance, that they could not have been adequately treated except by a man of the wide experience, scholarship and knowledge of general European archaeology possessed by the author.

Not that one can accept all his conclusions without demur. He says *e.g.* (p. 97) that no artifact 'indubitably worked with metal tools has been recognised at the site'. But among the relics are some thin perforated discs of bone or ivory which could hardly have been sawn without the use of metal; certainly none of the flint tools discovered appear capable of performing the operation. In a paper read before the Society of Antiquaries of Scotland in December 1930, Mr J. Graham Callander, Director of the Scottish National Museum of Antiquities, out of his unrivalled knowledge of Scottish prehistory, subjected Prof. Childe's evidence for a pre-bronch dating of the site to a most

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powerful and incisive criticism. But after careful study the present writer is bound to confess that he agrees with Prof. Childe, whose review of the relationship between the culture of the brochs and that of Skara Brae is a piece of singularly cogent, impressive and convincing argument. The total absence at Skara Brae of the most typical broch relics simply cannot be set aside.

Space forbids more than the barest mention of the amazing wealth of strange relics unearthed—the carved stone balls, the various other stone objects unique in form and workmanship, the Skara Brae bone 'adze', the wall-carvings and the rest. But in conclusion one must refer to the curious rarity of pig bones among those submitted to Prof. D. M. S. Watson for identification. One is forcibly reminded of Prof. W. J. Watson's theory as to the origin of the name—Orkney. Is the comparative absence of pig bones at Skara Brae due to the fact that the inhabitants were of the 'Orc' or Boar-folk tribe from whom Orkney takes its name, and that the boar was the tribal totem?

H. MARWICK.

LE PROBLÈME DES CENTAURES. *Par* GEORGES DUMÉZIL, D. ès L., Prof. d'histoire des religions à l'Université de Constantinople. *Paris: Geuthner, 1929.* pp. viii, 278. 75 francs.

This study of comparative mythology deals partly with folklore, partly with some modern survivals of primitive seasonal rituals, and attempts, not without success, to trace a connexion between the two. The author's first task is to describe traditional masquerades in which horses play a part, and he finds that the 'Gody' of Slavonic countries best preserve the form and sense of the ancient rites connected (a) with certain seasons of the changing year, and (b) with marriage. It is not necessary for us to go so far afield for a similar custom: the 'Mary Llwyd' exists at the present day in the Gower peninsula, where at Christmas time a party of mummers goes from house to house, one of them wrapped in a cloth and carrying above his head a horse's skull so arranged that the jaw can be moved by a cord.

Dr Dumézil collects analogous customs from the most remote parts of the world. The next thing is to examine myths of Indo-European peoples which present striking resemblances to the scenarios described in the opening chapters. After commending Mr J. C. Lawson for his identification of the modern Greek Kallikantzaroi with the Centaurs, the author goes further and finds that the Centaurs have relations in all Indo-European mythology, and the culmination of the work is the discovery of the close affinity of the Gandharva of Indian epic to the Centaurs of Greece, and the association of the former with the God of the New Year.

We may not follow Dr Dumézil in all his inferences: on the linguistic equation of *Gandharva* with *Kentauros* M. Meillet suspends judgment; but the book is interesting in itself and contains an invaluable store of references to the literature in all languages bearing on the subject.

J. F. DOBSON.

WEALDEN IRON: a monograph on the former Ironworks in the counties of Sussex Surrey and Kent. *By* ERNEST STRAKER. *G. Bell and Sons, 1931.* pp. xiv, 487. *With illustrations.* 15s.

That undulating, wooded plain, known as the Weald and forming parts of Sussex, Surrey and Kent, shares with the Forest of Dean the distinction of having been the principal scene of the iron industry of this country from prehistoric and Roman times

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down to the 18th century. The comprehensive study of this industry has been long overdue, and this lack is now amply supplied by the volume before us. It is probably the most complete account of ancient processes of smelting that has yet appeared and should be studied by all who in the excavation of ancient sites elsewhere find traces of this industry.

The preface contains a good epitome of the history of the Wealden iron industry. 'Unlike that in other districts, it was never able to substitute coal for charcoal as its fuel, and so came to a complete and definite end, not, as is popularly supposed, by the exhaustion of the woods, but from economic causes. Commencing in prehistoric times . . . the Romans, soon after their advent, exploited it on a large scale . . . After their departure, during the Dark and Middle Ages it appears to have declined in importance, but remained as a limited and almost domestic industry. In Tudor times, about the close of the 15th century, a new process was introduced from the Continent, and soon after the casting of iron cannon, at first by the help of foreign experts, was commenced in Sussex . . . In our island this was the first step of the change from a practically self-supporting and mainly agricultural community, exporting their surplus produce in an unmanufactured state, to a nation depending for the greater part of its sustenance on manufactured exports, and was intimately connected with the rise of overseas trade and colonisation' (p. vi).

Successive chapters deal clearly and ably with every aspect of this story. That on the geology of the Weald defines the limits of the district and the rocks, ferruginous and otherwise, of which it is composed. This is succeeded by a most important chapter in which the direct, or bloomery process of smelting is distinguished from the indirect, or blast-furnace, process, which was introduced about the close of the 15th century. The former, which was the only method in antiquity and which is still practised by native tribes, produced small pieces of wrought iron without melting the ore completely. By the later process large masses of wrought iron are produced by submitting 'sows' of cast iron to the action of large mechanical hammers, the latter being worked by water-power in the Weald.

Cast iron was unknown before the introduction of this process, unless accidentally produced. The kinds of slag, or cinder, resulting from these two processes are distinct and recognizable, and this is of great value when tracing sites in the field.

The early history is then outlined—early Iron Age, Roman, Saxon and early Medieval. Four pre-Roman and about ten Roman sites have been identified by the associated pottery. In the Wealden hill-fort of Saxonbury slag was pounded up for mixing in the paste of La Tène III pottery, and pieces of slag have been found in other Iron Age hill-forts. Some of the Roman works were exploited on an enormous scale; the cinder-heap at Beauport Park, near Battle, having originally been about 50 ft high, and extending over two acres of ground. When this material was removed between 1870 and 1880 for road-making considerable quantities of Roman pottery were found in the mound, including Samian ware, and also coins of Trajan and Hadrian, and other objects. At Oldlands, in Maresfield, very large quantities of slag were found, together with much pottery, some bearing potters' marks, and also coins of Nero, Vespasian, Tetricus and Diocletian, those of Vespasian being in the majority.

The evolution of the blast furnace is next described, largely from contemporary sources. The next three chapters trace the history of the industry from Tudor times to its extinction early in the 19th century. Then follow chapters giving more detailed accounts of the technicalities of the industry—hammer-ponds, furnaces, forges, mining

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and ore, fuel, and, most important from the point of view of field archaeology, cinders and slags. Bloomery slags are classified into seven types, illustrated by numerous photographs and micro-sections, and forge slags into two types; chemical analysis being given of each. The differences between the types is to a large extent due to slight differences in the processes of smelting.

Further chapters deal exhaustively with the products of the industry—the casting of guns, fire-backs and grave-slabs—the introduction of steel, the methods of transport, and the all-important question of finance.

One cannot conceive how the subject could have been dealt with more comprehensively or more readably. There is, however, one very disappointing chapter, which is unfortunately the first in the book. Though entitled 'Iron in General', this chapter is almost wholly given up to arguing that iron was known and worked, not only in the Bronze Age, but also in the neolithic period. That the use of iron was known at a very early period in Mesopotamia and Egypt is undoubted, but to apply such a view to Britain is quite another matter. The author puts forward no evidence of the use or knowledge of iron in the British Bronze Age, but argues that it must have been used because it is easier to work than bronze, and he accounts for its absence in Bronze Age deposits by supposing that it has rusted away! Such arguments need no comment. But the neolithic period suffers even greater misrepresentation. 'During the last few years', he says, 'evidence has been fast accumulating that iron was smelted in Sussex in neolithic times'. This astonishing statement is supported by three pieces of 'evidence'. (1) The discovery of iron slag by the present reviewer in South Down Camps is cited; but in each case it was found in Early Iron Age deposits, not neolithic.* (2) Mr J. E. Ray is stated to have found flint implements at bloomery sites; we also have found beer bottles at Cissbury, but never supposed that the one dated the other. (3) The discovery of several pieces of bloomery slag in the excavation of a neolithic settlement by Mr H. J. Cheney near Rye appears to be more formidable evidence. We have therefore looked into the matter personally, visiting the site in company with Mr Cheney, and reviewing the finds. The pieces of slag were found in a neolithic occupation stratum with definite neolithic pottery, under about a foot of top-soil. But in the same layer were found a dozen or more shards of equally definite medieval pottery which we would tentatively assign to about A.D. 1200. This therefore is the explanation of the slag, which, with the medieval pottery, is an intrusion into the neolithic occupation layer. Enough has been said to show that this chapter is crying out to be re-written, or omitted altogether.

The second half of the book consists of a topographical and descriptive survey of all the known iron-working sites in the Weald, arranged in 21 chapters according to river basins and their tributary streams. This is illustrated by 9 double-page maps (half inch to the mile) by John Bartholomew and Son, Ltd., overprinted in red and blue to show the positions of furnaces, forges, bloomeries, Roman bloomeries, and the average rainfall for 34 years. All this is extremely well done and valuable.

The illustrations, which are very numerous, mostly consist of photographs of sites as they appear at the present day. Some of these are full-page reproductions, but the great majority are reduced to about $2\frac{1}{4}$ by $1\frac{3}{4}$ inches, which not only detracts from the sumptuousness of the book, but does not always do full justice to the subjects represented.

* The reviewer must take his share of blame for having ignorantly described as 'slag' certain pieces of burnt pyrites found in neolithic levels at the Trundle in 1928.

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We feel that greater value for money might have been obtained by reproducing only half the number of photographs, and making them each about twice as large as they are.† Taken as a whole the book is admirably produced, and should be a standard for others to emulate. The type is clear, three different founts being used for the text and different classes of quotations. The adoption of marginal references instead of footnotes is refreshing, making reference easy, the reader's progress not being hindered by footnote numbers in the text. There are several minor misprints, all unimportant, so far as we have observed them. There are also a glossary of local terms, an index of parishes, and a very complete general index. Finally the cover is fully in keeping with a book which it is a pleasure to possess. Every dweller in the Weald should do so, as well as every dweller within motoring distance thereof, not to mention every lover of the history of England's greatness.

E. CECIL CURWEN.

SCHLIEMANN OF TROY: the story of a goldseeker. By EMIL LUDWIG. *With an introduction by SIR ARTHUR EVANS.* Putnam, 1931. pp. 336. 21s.

Schliemann was one of the great discoverers of the 19th century, if not of all time, and yet in the forty years that have elapsed since his death no account of his life has appeared although it was well known that he left the most copious material behind him. An autobiography in the introduction to his *Ilios* is the only connected account of his life hitherto available. Now his widow, son, and daughter have placed his diaries, journals, and letter books at the disposal of Dr Ludwig, who has added Schliemann to his series of biographies of Napoleon, Bismarck, and other heroes. He has painted a wonderfully vivid picture of the self-made business man who made himself also an archaeological excavator. Schliemann's energy, industry, adaptability, amazing capacity for learning languages, careful attention to detail and to method made him almost an ideal man of affairs. His other characteristics, more prominent perhaps in his private life, which is tactfully handled by his biographer, seem to have influenced his business less than his archaeological career. He could be impulsive, impatient, ready to take offence, and yet at the same time generous, romantic, considerate, and loyal. Here in his business and in his private life Dr Ludwig's account is based on documents generally inaccessible and written in a dozen different languages, and must be accepted especially since it is authorized by Schliemann's family. Between the main outlines already known the author fills in the details with strong contrasting colours. Of special value is the charming portrait of Madame Schliemann, who in the author's sympathetic treatment appears in her right place as the true and loving partner of her famous husband's work, joys, and sorrows.

On the archaeological side, however, where he can be tested by independent evidence, the author is less happy and hardly does justice to Schliemann's achievements. Nor does he seem, to me at least, right in considering 'gold' as the mainspring of his life. Schliemann's passion for Homer and his dream of establishing the historical basis of Greek heroic legends by the unearthing of material archaeological facts seem inconsistent with the author's 'gold' motive. He hardly appears to have been a mere treasure hunter, or one of those archaeologists who are credited with digging mainly for loot. If he had been, even in spite of his romantic tendency he could hardly have written 'nothing more

† We whole-heartedly agree. When *will* publishers learn to prefer quality to quantity in illustrations?—EDITOR.

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interesting can be imagined than the excavation of a prehistoric city of immortal glory where nearly every object, even to the fragments of pottery, reveals a new page of history'. In his publications on Mycenae and Troy there is no excessive jubilation over the gold as gold. In laying emphasis on the archaeological value of pottery he was anticipating modern scientific archaeology. He was looking much more for Homeric parallels and for proof of the historical value of Greek tradition. Here again modern ideas have justified his attitude. The author, in saying that at Mycenae Schliemann removed the grave stelai without determining their positions or their relationship to the graves, is not quite correct. For four years during the recent British excavations at Mycenae I had the opportunity of reading Schliemann's account of his excavations again and again on the actual scene of his labours, and also, thanks to Madame Schliemann's kindness, was able to make free use of his original notebook written from day to day in 1876. From such personal observation and from the results of our own investigations on the stratification of that complicated site, the Grave Circle at Mycenae, Schliemann appears as an excavator in most respects no worse, and in several better, than his contemporaries who had had archaeological training. His businesslike common sense and his acuteness in observation led him to make notes on the stratification in a manner which was not followed by others in excavations of a decade or more later. In his publications Schliemann gave sections, noted the depth at which he found the individual objects, and also gave the scale of his reproductions, points often overlooked even today by archaeological professors who attempt excavation. Schliemann's observations on practical points of this nature should be given serious consideration. At a time when the excavators of Olympia were content not to explore whether the Heraeum was really the earliest temple on that spot, he acted on the principle of digging to virgin soil to find the whole history of a site. If in this he was apt, as in the great north trench at Troy, to be too drastic, his method was not much worse than some of those employed in this century by professional archaeologists. Impatience and impulsiveness, Schliemann's two worst faults as an excavator, are not peculiar to him alone. He and his collaborator Dörpfeld were right in recognizing the palace at Tiryns as a prehistoric building, against the contrary opinion of Penrose. Here his common sense and reliance on the facts of excavation were fully justified. The absurd criticisms expressed about the treasures at Mycenae or by Bötticher on the character of Troy have gone the way of similar futilities.

Thus, in a book so fascinating as this biography of the errand boy who became a merchant prince, and then the discoverer of the great Bronze Age civilization of the Aegean, one could wish that the author had enlisted archaeological aid to enable him to give in parts IV and V the proper value to Schliemann's work as an excavator in the light of the most modern research on Mycenae, Tiryns, and Troy. A. J. B. WACE.

THE SCULPTURE OF THE NIKE TEMPLE PARAPET. By RHYS CARPENTER, with photographs by BERNARD ASHMOLE. Cambridge, Mass.: Harvard University Press, 1929. pp. 83, with 34 plates, 15 figures, and plan. \$2.

Designed by the architect of the Parthenon, for twenty-one centuries the little temple of 'Wingless Victory' stood intact upon the bastion of the Acropolis. Pulled down by the Turks to make a bastion of their own, it was reconstructed by Ross in 1835, and is now familiar to all who visit Athens. Surrounding it on three sides there used to be a low parapet, or 'balustrade', carved with figures of Victory and surmounted

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by a metal grille. The fragmentary remains of this parapet have received much attention from scholars—Kekulé, Heberdey, Dinsmoor especially—and are very familiar to students of Greek sculpture as masterpieces of the late fifth century. They illustrate strikingly that mastery in the representation of drapery and in the creation of the illusion of transparency which is characteristic of their period: the most famous of these fragments is that which shows a Nike unbinding her sandal. Professor Carpenter in this book gives us a definitive study of his own researches, which may be said to have crowned the work of those who have preceded him with all but finality: 'all but', since minor problems remain, and there is still hope that a few more fragments may yet be found. Professor Ashmole's share in the work is such as those who know his skill in this kind of photography will expect: and the printing was entrusted to the Spamersche Buchdruckerei, of Leipzig.

Professor Carpenter makes a close stylistic analysis of the fragments, in the order in which they stood; this is illustrated by the photographs showing not only individual slabs, but also details of the technique upon which the conclusions are based. It is thus possible for the reader not only to see for himself the justice of the conclusions, but also to enjoy a lesson in criticism whose value it is difficult to over-estimate. The balustrade is shown to have been the work of six sculptors, whose unequal merit is assessed by reference to their preference for chisel or drill (the use of the 'semi-running' drill became common at this time), their temperament and their command of technique. He suggests, with probability, that they divided the work equally, except that the chief of them made in addition to the half-side which fell to each man's share the little extra portion which forms a short return upon the eastern flank: he thus carved ten figures, each of the others eight. Professor Carpenter wisely prefers to leave these sculptors nameless: but he suggests that in Master 'A' we may recognize the over-anxious Kallimachos, whose sheer skill and passion for perfection tempted him to over-elaboration, as Pliny tells us; while Paionios of Mende, the maker of a yet more celebrated Nike, is most plausibly suggested, by a detailed and illustrated comparison of the drapery of the two works, as identical with Master 'B', and the affinities of Master 'E' with the originator of the Venus Genetrix type are again demonstrated by comparative photographs. But in fact names are of secondary importance; we have the sculptures, sadly fragmentary indeed, but telling their own tale.

W. L. CUTTLE.

THE TREASURIES OF THE GREEKS AND ROMANS. By H. N. COUCH.
Menasha, Wisconsin: The George Banta Publishing Company, 1929. pp. 111.
Cloth 10s. 6d., paper, 6s 6d.

The writer discusses everything that the Greeks and Romans called 'treasuries'; strong-rooms, *tholos* tombs, prisons, money-boxes. His manner is somewhat laboured: so much proof is not required to convince us of the sepulchral use of the Mycenae *tholoi* (for his treatment of which Sir Arthur Evans' latest contribution to the problem of these and the Shaft-Graves' interconnexion had not yet appeared), even if Pausanias does call them treasuries. The fact is that if we translate the word 'keep'—a rendering which agrees well enough with the conservative derivation from *τίθημι* to which the writer adheres, while wisely reserving the right to think that there may be a pre-Hellenic history behind it—we shall not be far wrong. Too much significance is attached to the partial burying of the *pitthos* in which Eurystheus is shown as imprisoned: that is the way to use a *pitthos*. And when Apollodorus says it was a bronze *pitthos*, he is only

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behaving in the traditional epic manner, which exaggerated the bronziness of the Bronze Age.

The latter part of the book, which deals with temple treasuries and private safes, includes a useful discussion of the nature of the *opisthodomos* in which the Athenian public wealth was stored.

The pursuit of this word is interesting; I have been curious about the 'coin-in-the-slot machine which sold holy water' ever since I noticed that meaning in Liddell and Scott, but this Thesaurus of Thesauri leaves me still curious on that particular point.

W. L. CUTTLE.

HOLT, DENBIGHSHIRE: the works depôt of the twentieth legion. By W. F. GRIMES. *Y Cymmrodor*, vol. XLI, pp. 235, 80 figs. Issued by the Society, New Stone Buildings, 64 Chancery Lane, 1930.

In 1907 the late T. A. Acton, F.S.A., undertook, at his own expense, the excavation of what proved to be the pottery works-depôt of the 20th Legion at Holt, about 10 miles from Chester. The work was continued until 1915. Ten years later no report had appeared, when the material was acquired by the National Museum of Wales. The arrangement whereby Mr Acton was to have worked over it at Cardiff was upset by his illness and death in 1925. The task of preparing the report was then undertaken by Mr W. F. Grimes. The difficulties to be overcome were very great. Not only was it found impossible to trace any notes made by the excavator, but there was an almost complete absence of labels on the very numerous finds or of records of their stratification; even some of the original plans seem to have disappeared. Let it be said at once that Mr Grimes has to a remarkable degree surmounted these difficulties and has saved from oblivion a site which is unique in Roman Britain.

The meagreness of the evidence has naturally affected his treatment. The pottery which should have provided a dated series of prime importance has had to be classified on typological grounds alone. The description of the buildings is necessarily lacking in detail. Their main features are, however, clearly described and illustrated by excellent plans and sections drawn by the author. They consist of a workman's barracks, a bath-house, and a small corridor-house. More important are the industrial buildings—two sets of workshops and a drying-shed, a large double-flue pottery kiln, and the main plant, consisting of a range of five (originally six) tile kilns and two pottery kilns. The bath house contained a hypocaust of unique type, shallow channels in the floor being formed of a triple row of box tiles. If these really were flues and not ventilating channels similar to those recently found in the tepidaria at Mumrills, they must have been so inefficient as to be only explainable as the work of an amateur architect under orders to make use of a number of unwanted box tiles!

The dating of the activities of this industrial plant is vaguer than it would have been, had proper records of the excavation survived, but it seems clear that it was established towards the end of the first century, to provide the tiles required for the re-building in stone of the legionary fortress of Chester. Its occupation was intensive during the early second century, fell off during the Antonine period, when the military forces of Wales were transferred to the northern frontier, and saw a revival of activity early in the third century. The date of its final abandonment cannot be fixed.

Section IV of the report deserves special attention. In this the author has attempted an illustrated classification of Romano-British kiln types, followed by an annotated

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topographical list of kiln sites. Until recently the meagre and often inadequate records of such sites were buried in the journals of local archaeological societies. The future investigator of this hitherto neglected branch of Romano-British industry will have cause to thank Mr Grimes for his pioneer work.

The report is beautifully produced and printed and admirably illustrated by good half-tone blocks from photographs taken during the excavations and by numerous drawings by the author, which are models of what such drawings should be. The thanks of all students of Roman Britain are due to the Honourable Society of Cymmrodorion for a publication worthy of so important a site and to the writer for so ably surmounting difficulties that should never have arisen.

PHILIP CORDER.

THE ROMAN FORT AT MUMRILLS NEAR FALKIRK. By Sir GEORGE MACDONALD and ALEXANDER O. CURLE. *Proc. Soc. Antiquaries of Scotland*, LXIII, 396-575.

This valuable report gives the result of work carried out during four and a half years on the fort at Mumrills, on the Antonine Wall. In 1923 the site was included in a housing scheme, its examination thus becoming urgent. The illustrations show that the work was one of peculiar difficulty, but the skill and patience of the excavators have not only recovered the history of the site, but, owing to their discoveries in connexion with the two bath-houses, have produced a report that is indispensable to the student of Roman Britain.

The history of the site begins with a short Agricolan occupation to the west of the later Antonine fort. This early fort, of which slight but convincing traces were found, had the unusually large area of 6 acres, more than ten times that of the contemporary fort at Bar Hill, which suggests that it may have formed the headquarters of the officer in command. The Antonine fort is also much larger than any known station on the Antonine Wall. That the commandant's house, at first a large wooden building, was reconstructed, after being burnt, on a more ambitious scale in stone, suggests that the fort was once again selected as the headquarters of the officer in command of the Wall. Three periods were distinguished in this and in the headquarters building, confirming the generally accepted view that the Antonine Wall was twice destroyed before its abandonment. The relative scarcity of finds on this site, in contrast with the rich harvest at Newstead, is explained by the hypothesis that the final withdrawal of the garrison was here deliberate and orderly.

Two bath-houses were found within the ramparts—a small building in the northeast corner, probably for the use of the men, and a larger establishment that formed the southeast corner of the commandant's house after its reconstruction in stone. The section of the report dealing with the hypocaust arrangements in these two buildings is an original contribution to Roman studies of first rate importance. The discussion of the probable mode of operation of the channelled hypocaust in the men's bath-house (p. 460), and of the ventilating channels beneath the pillared floors of the tepidaria and caldarium in the large bath-house (p. 483) will form a guide to all future excavators and will lead to the reinterpretation of many past excavations. Indeed his discoveries have already led Sir George Macdonald to re-examine the bath-house of Chesters, the first intelligible account of which he gives in the current volume of *Archaeologia Aeliana*.

The authorship of the report is sufficient guarantee of its general excellence. In some respects, however, some of the illustrations are unworthy of the text, with its

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lucid exposition aided by the excellent plans. The half-tones, from photographs clearly taken in bad weather conditions, are too often poor (figs. 22, 59 and 65, for instance). Opinions will continue to differ as to the best method of illustrating decorated Samian ware. The pencil drawings here used have great artistic merit, but nothing can be said in favour of the so-called 'elevational' drawing of a Dr. 37 (fig. 77, no. 8), in which it is represented as a Dr. 30. Much of the coarse pottery, though shown in section, is drawn in perspective, so that the exact slope of the side of a vessel is not certainly indicated. When this method is combined in a single drawing (fig. 105, no. 9) with the 'ideal section' method, now universally adopted in good excavation reports, faith in its accuracy is severely shaken.

PHILIP CORDER.

EXPLORATION ARCHÉOLOGIQUE DE DÉLOS : faite par l'École française d'Athènes. Fascicule XI : les sanctuaires et les cultes du mont Cynthe. Par ANDRÉ PLASSART. Paris : E. de Boccard. pp. 320 with 6 plates and 257 figs.

Every Hellenic traveller who visits Delos climbs Mount Cynthus to enjoy the panorama of the island and of the Cyclades to be seen thence and also on his way to study the sacred grotto long supposed to be the birth-cave of Apollo and his twin sister. Those who have seen Delos more than once in recent years will have remarked with satisfaction the methodical progress of the French archaeologists towards a complete exploration of the whole mountain. This has now been achieved by Professor Plassart and in this volume, which is in scholarship, printing, and illustration worthy of its subject, he describes and discusses in detail the results obtained by himself and his colleagues. The text is clear and concise, and he adds much valuable material to our knowledge of Delos from all points of view, archaeological, historical, and religious. The only thing lacking is a full index. The author begins with the summit and its first inhabitation, which dates as far back in the Bronze Age as the last centuries of the third millennium B.C. There are house foundations, stone implements, obsidian flakes, and fragments of Cycladic and Mycenaean pottery. He proceeds with the sanctuaries of Zeus and Athena which next occupied the summit, and describes them chronologically through archaic times and the periods of Delian independence and of the second Athenian domination. He then comes to the western slope and the extremely interesting precinct of Hera. Here a small seventh century shrine was in the sixth century completely surrounded and hidden by a small marble Doric temple, distyle in antis, which is worth attention for the slender proportions of its architecture. In the early temple thus buried was found a wonderful hoard of archaic votives of all kinds, some of which make the identification certain by their inscribed dedications to Hera. Of this important collection a special account is given in a separate volume by M. Dugas. Before the temple stood an altar. A road which leads up the northwest side of the mount approaches the sacred cave. After a long and careful discussion of all the circumstances the author concludes that it was a shrine of Heracles and according to the pottery, sculpture and other finds dates it to the Hellenistic age, probably the third century B.C. He is unable to find any definite evidence that it was a primitive or archaic sanctuary which had been restored and reused. The last sides of the mount to be described are the northern and eastern, where with some nameless sanctuaries are those of the Gods of Ascalon, of Zeus Hypsistos, of Eileithyia and the rock of Leto. In a short summary some of the more important points are indicated. First comes the prehistoric age with the traditional connexion of Minos with the islands and of the

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'Carians' with Delos. The sanctuaries on the sacred mount which gave Artemis her favourite epithet Cynthia prove the popularity of Delos in the Greek world and the inscriptions and other dedications show how widespread was the reputation of Apollo's island when in the Hellenistic age the bounds of that world were enlarged. It was honoured by Ptolemy Philadelphus and Arsinoë. The shrine of the gods of Ascalon, and votives from citizens of Gerrha, Seleuceia ad Tigrim, Gaza, and Jamneia attest the fame of Delos in the Levant. Zeus Hysistos may well have been Baal and an Arab from the Hadramaut engraved in his own language a dedication to the moon god. Thus a study of Cynthus gives an epitome of the history of one of the great panhellenic shrines and the far reaching influence of Greek culture in Hellenistic days at least.

A. J. B. WACE.

ARCHÄOLOGISCHE ENTDECKUNGEN IM 20 JAHRHUNDERT. By FRIEDRICH VON OPPELN-BRONIKOWSKI. Berlin: Heinrich Keller, 1931. pp. 165, with 40 illustrations. 4.50 Rm.

This attractive little book is intended as a supplement to the well-known work of Michaelis, *A Century of Archaeological Discovery*, published in 1903. It is designed to give the German public an idea of the extent and nature of the archaeological discoveries which have been made since the beginning of the present century. The author modestly disclaims the title of 'professional archaeologist': but he has travelled widely, and he speaks from first-hand knowledge. As might be expected in the work of a German, writing for Germans, most of the researches which he describes are those of German excavators: but by no means exclusively so. Full justice, for example, is given to the work of Woolley at Ur. (By a slip, the initials of his lamented predecessor H. R. Hall are reversed). The five chapters of the book are devoted in turn to Mesopotamia, Syria, Asia Minor, Egypt, Classical Archaeology, the Romans in Germany, Prehistoric and Early Historic Germany. For the English reader, perhaps, the last two chapters are the most useful; the sensational discoveries made in the great civilizations of the near East are rendered familiar by easily accessible publications; but the wonderful work which German scholars have recently accomplished, in elucidating the Archaeology of their own land, has to be sought for in a mountain of periodical literature. The bibliographical references given in this book will aid the search considerably.

A few of the illustrations, representing the restorations of ancient buildings, are perspective drawings; the rest are all reproductions of photographs. One of these, that of the temple of Zoser at Sakkara, seems to be slightly over-exposed, and in consequence is a little flat; but the rest are uniformly excellent, both in their photographic printing and in their reproduction as half-tone plates.

R. A. S. MACALISTER.

MERCHANT VENTURERS IN BRONZE. By HAROLD PEAKE and HERBERT JOHN FLEURE. Oxford: Clarendon Press (Milford), 1931. pp. vii, 168, with 67 illustrations. 5s.

This is the seventh, and in many respects the most interesting, of the invaluable 'Corridors of Time' series, which is by now well established as the most convenient short summary of cultural history available in English. Parenthetically, the 'hole-and-corner' suggestiveness of the word 'corridors' always strikes at least one admirer of the series as misplaced: 'Broad Highways of Time' would seem to him a more appropriate designation!

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None of the volumes of the series is an 'arm-chair' book. The amount of material compressed into their narrow compass is astonishing, and as an inevitable consequence, very close attention is needed in studying them. The present volume is worthy to stand beside its predecessors; and this is high praise.

Its subject, generally speaking, is the Bronze Age. The authors convey their readers in a swift survey of the field from Ireland to China: the student who diligently follows them will have laid a sound basis for an accurate knowledge of the period, and for an understanding of the processes of reasoning whereby forgotten, unrecorded history can be recovered from uninscribed relics.

Being a human production, the book is not wholly faultless. The illustrations, on the whole, are the weakest feature. They are well selected, but some of the drawings are scratchy, and certain of the photographs (especially those on pp. 39, 145) are printed rather too black for clearness. Photographs would have been more satisfactory than the not very pleasing drawings of ethnological types on pp. 5, 11. Surely a better likeness of a broad-headed German could have been obtained than the old woodcut—little better than a caricature—of Martin Luther: and we doubt the desirability of inserting named portraits of well-known living persons, and turning them into ethnological specimens. On p. 3 we note the old mistake of treating Ofnet as a *place* ('from Ofnet in Bavaria'). The word, which literally means a baking-oven, is in the local patois applied to a rock hollow: the Great and the Little Ofnet are *caves*, and should be referred to as such. The La Tène collar from Broughter should not appear on a plate with a caption assigning the objects figured upon it to the Bronze Age (p. 21). The fanciful name 'sun-disk' applied to another object on the same plate might with advantage be quietly dropped. There is no sense in it, and the analogy with the Trundholm disk, which has suggested it, is wholly superficial.

On the other hand the book contains many good points, among which we may mention the very probable relation suggested between the tholos graves and the shaft graves of Mycenae; the recognition that there is no water-tight compartment between the Neolithic and the Bronze Ages; the emphasis laid upon the 'secretiveness of inventors' as a source of complication in cultural history; and the admission of the possibility of independent inventions in different countries—a welcome ray of hope that we are at last being delivered from the Pan-Egyptian nightmare.

R. A. S. MACALISTER.

THE HISTORY OF THE MAYA. By THOMAS GANN and J. ERIC THOMPSON.
Charles Scribner's Sons, 1931. pp. 264. 8s 6d.

The collaborators in this history have hardly attempted coordination, as they confess. Some of the chapters Dr Gann wrote in Belize, the while, remotely in Chicago, Mr Thompson wrote others. As result, there is occasionally a refreshing disagreement—one author blandly and unwittingly refuting the statements of his colleague's previous chapter. Variety—if not gaiety—is added at the expense of conciseness.

Dr Gann traces the origins of the Maya civilization to the culture of the 'Archaic horizon' first defined by Dr Spinden. Modern research has shown both culture and 'horizon' as more imaginative concepts than definitions. Bringing the Maya colonizers of the Old Empire from Huastlan Dr Gann also marches with the orthodox, though there is no trace whatever of such a migration. To site the Tuxtla statuette is irrelevant: it may well have been an export from the Old Empire region. And, while among

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irrelevancies, it may be pointed out that there is the best of traditional evidence on the side of the assumption that the Teotihuacan pyramids are Otomi, not Toltec work.

Dr Gann gives a brief sketch of the coming into being of the Old Empire, its arts, crafts and sciences. He disbelieves in any sudden catastrophic evacuation of the historic territory, holding that Yucatan had meantime been fairly thoroughly colonized and that the drift of the Maya into that territory was no unpremeditated exodus. The Bacalar region apart, there is still no evidence of any such colonization, for neither the Cobá nor Tulum dates can be ascribed with any certainty to the tenth cycle.

Mr Thompson outlines the history of the New Empire, and, apart from the usual confusion of Xiu with Itza, the outline is of value.

The accounts of the religious ceremonies, daily life, war organizations and calendrical attainments of the Maya are admirable, and, like the earlier sections, skilfully illustrated.

J. LESLIE MITCHELL.

THE MIRROR OF EGYPT IN THE OLD TESTAMENT. By LIEUT. COMMANDER VICTOR L. TRUMPER, R.N.R. *London and Edinburgh: Marshall, Morgan, and Scott. pp. 174, and 7 plates. 5s.*

Commander Trumper's book is intended for those 'who have the slightest desire for a fuller understanding of Scripture, and who may be interested in the land of Egypt, where the relics of antiquity throw such a vivid light on the allusions to Egypt in Holy Writ'. It is essentially, therefore, a book written by a layman for laymen and, within these limits, generously fulfils its purpose. The author has lived for more than twenty years in Egypt, and his observations of its life and customs, coupled with a keen interest in matters Biblical and archaeological, have helped him to appreciate the local colouring of the Old Testament stories. Those who wish to know how bakeries were organized in ancient days, how bricks were made, why Joseph was given royal linen, why silver is put before gold, and many other interesting sidelights on the narrative, will find here, together with a number of excellent photographs, much varied and unfamiliar information. It is a pity that the author, having decided to eschew dates, should have committed himself after several hesitant qualifications to the 'Ramses II' theory of the Oppression. Not only is this theory now widely challenged, but the view which makes Amenhotep II the Pharaoh of the Exodus is not without its own interest to the hunter of side-lights. We may, in fact, make Commander Trumper some return for an entertaining book if we refer him, *more ipsius*, to Breasted's History of Egypt (2nd ed., p. 327). He will find there a suggestion that Thothmes IV, who immediately followed Amenhotep II, *was not his father's firstborn and had at one time no expectation of succeeding to the throne*!

W. J. PHYTHIAN-ADAMS.

YESTERDAY AND TODAY IN SINAI. By MAJOR C. S. JARVIS. *Edinburgh: Blackwood, 1931. pp. 312 and 28 plates, 1 map. 15s.*

It is difficult to resist the temptation of quoting at large from the many entertaining and instructive things which the Governor of Sinai has to record about the 'Today' of that little-known peninsula; its sport, its smuggling, its locust-wars, its tribal laws and customs, and always, and not least, its round 25,000 of incorrigibly happy-go-lucky Beduin. Space, however, limits us to the 'Yesterday' which has made this region famous, and, more particularly, to the author's theory of the forty years' wandering of the Israelites. Very few scholars today maintain the traditional identification of Mt. Sinai with Gebel Mousa in the extreme south of the peninsula: and Major Jarvis brings

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further arguments against it out of the stores of a firsthand knowledge of the country. He himself places Mt. Sinai tentatively at Gebal Hellal 30 miles south of El Arish, and would refer the scene of the wanderings to the triangle of cultivable land between that town, Rafa, and El Kosseima. In harmony with this view, he advances the not altogether new theory that the 'Red Sea' of the Bible (the Hebrew 'Yam Suph' or 'Sea of reeds') was in reality the Bardawil Lake, a reedy lagoon on the Mediterranean coast not far east of Port Said. He suggests that it was along the narrow strip of land which divides this lake from the sea that Moses and the Israelites made their escape in safety, while the pursuing Egyptians were bogged by an inrush of sea, caused by the subsidence of the low sand-bar under a heavy east wind. Many critics, from Welhausen onwards, have urged that the real scene of the law-giving and the chief 'miraculous' events of the wanderings was to be sought in the vicinity of Kadesh Barnea (which Major Jarvis identifies with Ain El Gedeirat 3 miles east of Kosseima rather than with the more commonly accepted Ain El Kudeis): and some (e.g. Kittel) have wished to locate Sinai in the same neighbourhood. It is, therefore, of great interest to find this view put forward independently by one who, as an administrator, has had the opportunity of studying the problems so closely on its purely practical side. It is perhaps not possible to accept all his conclusions. His identification of the 'Yam Suph', for example, is open to the serious objection that whatever the original meaning of this name may have been, it was undoubtedly applied in the time of the monarchy to the Gulf of Akaba. (Curiously enough, the author notes a Mohammedan belief that 'the disaster occurred on the opposite side of Sinai in the Gulf of Akaba'. We should have been grateful here for references). Moreover, the careful distinction between the 'Way of the land of the Philistines' and the 'Way of the wilderness of the Yam Suph', the latter being chosen by Moses though the former was 'near', can scarcely be reconciled with the inconsiderable distance which separates the two routes necessitated by this theory. Of special interest is the author's suggested explanation of the Pillar of Cloud and Fire. He notes the occurrence in Sinai, when heavy weather is impending, of a remarkable cloud-formation, 'a huge column of cumulus, black in the centre with white edges. This column, which begins on the skyline and is most impressive, extends to the zenith, constantly emitting lightning, and at night is an intermittent blaze of fire'. Here again it may be asked whether such a cumulus 'coming in from the eastward' and presumably passing away over the Mediterranean, will satisfy the Biblical description of the Pillar which went before the Israelites and guided them on their long march. It may be unwise, however, to press every detail of an ancient tradition too closely; and we are grateful for a real effort to grapple with this enigma, which, apart from Gressmann's volcanic hypothesis, has not always been treated so respectfully. On the subject of the quails and the tamarisk-manna the Governor of Sinai may claim to write with authority, and we have little doubt that his conclusions on both these points will be endorsed by future research. His practical contribution to Biblical archaeology will be welcomed by students at home, but it is to be hoped that in succeeding editions he will not allow such forms as Kadish, Succouth, and Goschen to remain uncorrected.

W. J. PHYTHIAN-ADAMS.

VORGESCHICHTLICHES JAHRBUCH: BAND IV; BIBLIOGRAPHIE DES JAHRES, 1927. Berlin: Walter de Gruyter, 1930. *ph.* 420.

The delay in the appearance of this volume (it only reached ANTIQUITY in December 1930) is explained by the sudden death of the Editor, Prof. Max Ebert, wherein prehistory

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lost not only a great savant but an exceptionally gifted organizer. For this volume he had in most cases secured the services of eminent local authorities like Baron Loë (Belgium), Kostrzewski (Poland) and Björn (Norway) to compile the bibliographies of their respective countries.

The *Jahrbücher* are intended as supplements to the *Reallexikon*, keeping that work up to date by summarizing the results of investigations published since its appearance. The volume before us gives a very admirable and exhaustive survey of books and articles dealing with prehistory in most European countries (Austria, France and the Iberian Peninsula are omitted this year) and Hither Asia. The section on the British Isles is this time very complete and well-balanced. The heading 'Russia' affords a glimpse of the enormous mass of literature, some of it obviously of first-class importance to the world at large, which is being produced in the Soviet Union but is apparently absolutely unknown and unobtainable here; it is tantalizing that only the titles, happily in Cyrillic, with a German translation, but scarcely a hint of the content, should be given.

By reading through the relevant section of this volume the worker interested in any given area will be surprised at the gaps in his own knowledge but will be able to pick out precisely what he must read to fill them. More can hardly be expected; to make the bibliography strictly a supplement to the *Reallexikon* a very exhaustive subject-index, which must take years to prepare, would be needed. The existing index gives only the names of the authors whose works are cited and seems otiose. In addition to the bibliography, there is an article by Ungnad on the significance of the Royal Tombs at Ur, which is perhaps better on the philological than on the archaeological side, and a very handy account of the principal Swiss museums by Tschumi. It is altogether to be hoped that this indispensable bibliographical work may be continued.

V. G. CHILDE.

DIE ARTEN DER KÜNSTLICHEN SCHÄDELDEFORMATION. By J. IMBELLONI. *Anthropos*, vol. xxv, 1930. **INTORNO AI CRANI 'INCREDIBILI' DEGLI INDIANI NATCHEZ:** Atti del xxii Congresso Internaz. degli Americanisti, Rome, Sept. 1926. By J. IMBELLONI.

In his paper in *Anthropos* Prof. Imbelloni has enlarged the ideas expressed in the slight sketch read to the Congress of Americanists. This was the more necessary as the latter contained numerous serious misprints. The paper in *Anthropos* includes a series of very beautiful photographs of artificially deformed skulls. The author's method of classifying these is by taking tracings of the skull and recording the angles from various points, but it is somewhat unfortunate that at least one of his points cannot be actually seen from the aspect in which he makes his tracing. Except for three or four modern references and a bibliography of his own writings he has paid very little, if any, attention to the modern work on this most important and interesting subject.

L. H. DUDLEY BUXTON.

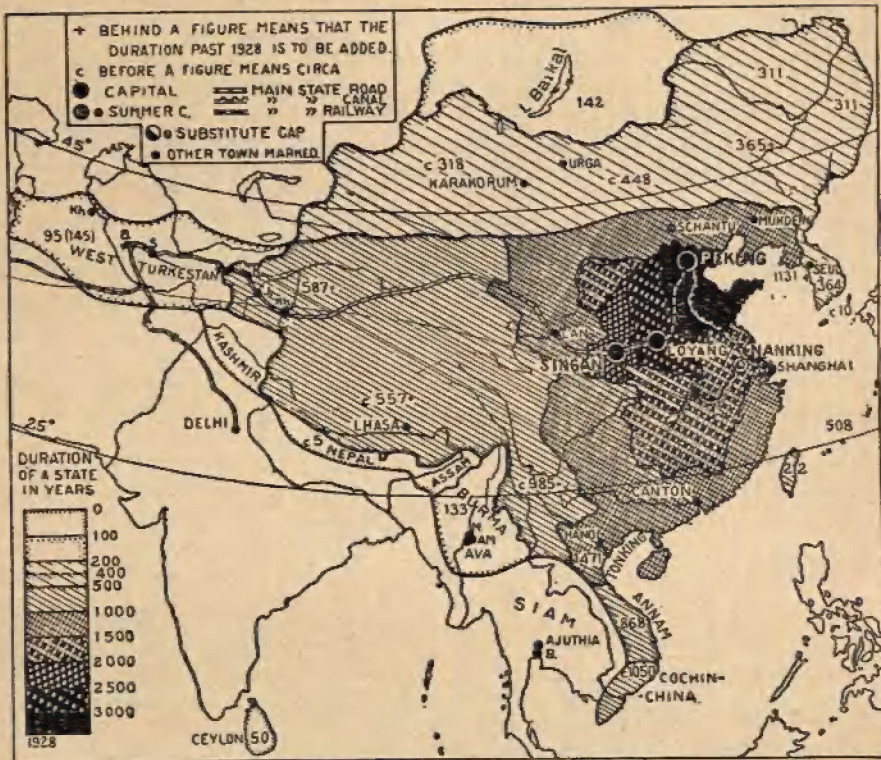
THE SUBTROPICAL BELT OF OLD WORLD EMPIRES. By STEN DE GEER. *Geografiska Annaler*, 1928, pp. 205-44. Stockholm: Centraltryckeriet, 1929.

This is a valuable and stimulating article. It belongs, as the author states, to the 'new political geography' which is succeeding the old, after a transitional welter of 'human geography'. He shows that the Old World Empires have originated and developed mainly within a subtropical belt lying between lat. 25° and 45° north. The

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frontiers of this belt may also be defined—on the north by the limit of vine-growing, and on the south by 'the isotherm of a fairly cool January (60° F or 15° C) . . . The central line of the belt may be fixed as the northern limit of palms . . . It runs most significantly past Cordoba, Rome, between Constantinople and Memphis, through the group of Mesopotamian and Persian capitals, between Ghazni and Delhi, though near both of them, finally past Nanking and Kyoto' (p. 243).

The expansion and contraction of these Empires is illustrated by fourteen sketch-maps on a uniform scale of 1:40,000,000, and by a folding map of the whole area, full



The Chinese Empire. Scale 1:40,000,000. (After De Geer)

of the most fascinating correlations but badly overcrowded and therefore almost illegible. The chief feature of these maps is the attempt to indicate the time-element of expansion and contraction by a system of shading, the regions being separated by 'isochronic' lines (the term is our own invention). Thus the problem of the fourth dimension, always so difficult for the historical geographer, is circumvented. When possible the main artery of each Empire and the capital cities are shown.

Perhaps the two most striking maps are those here reproduced (by permission), of the Chinese and Roman Empires. The nuclear area where political power originated and survived longest—the political centre of the Empire—is shaded darkest; it is these nuclear areas that are most important of course.

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In so all-embracing an essay minor discrepancies are inevitable. It seems unnecessary to particularize the Assyrian Empire as 'founded on force'. It was, of course, but then so is every other Empire. The 'more or less regular raiding and sacking' of the border provinces differed from other imperial exploitations only in being more open and clumsy and therefore less effective in the long run.

It is interesting to find that Egypt is 'closely bound to the other Mediterranean and East Mediterranean countries' rather than to the 'West Asiatic centre of Empires' (p. 231). This conclusion, derived quite independently from geographical study,



The Roman Empire. Scale 1 : 40,000,000. (After De Geer)

agrees entirely with Sir Flinders Petrie's conclusion from the historical view-point.* Indeed Mr De Geer almost restates it when he says that 'the area of East Mediterranean States and civilization has for several centuries been designated by the regional name the Levant. It has long been considered a national unit'. A treatment of the wave theory of civilization on geographical lines analogous to these followed here by Mr De Geer would be very illuminating, and we hope that some day he may undertake it. Only by work on such lines can broad generalizations be established; and it is only by means of broad generalizations that history can be rescued from pedants.

* 'The phase of the wave of civilization was identical in Egypt and Europe to within a century, where it can be observed in three periods . . . The Mediterranean and Egypt, as a whole, form therefore a single group in the history of civilization'. (*Revolutions of Civilization*, p. 83).

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THE TOPOGRAPHY AND HISTORY OF BETH-SHAN. By ALAN ROWE.
Philadelphia: University Press, 1930.

THE FOUR CANAANITE TEMPLES OF BETH-SHAN: part II, The Pottery.
By G. M. FITZGERALD. *ib.*

BETH-SHAN EXCAVATIONS, 1921-1923: the Arab and Byzantine Levels. By
G. M. FITZGERALD. *Philadelphia: University Press, 1931.*

THE THIRD WALL OF JERUSALEM. By E. L. SUKENIK and L. A. MAYER.
Jerusalem: University Press. London: Oxford University Press, 1930. 15s.

The publications before us are most welcome additions to the literature of research in Palestine, though in these hard times we cannot help asking whether it was necessary to publish them on so magnificent a scale. The Beth-Shan volumes are only the first fragmentary instalments of a final publication of one of the greatest excavations in the country, and we tremble to think how many other volumes will be required if this standard is maintained. 'The Third Wall of Jerusalem', on the other hand, is an account of what was in itself only a small undertaking which the authors were debarred by want of funds from completing as they wished. It is a much less sumptuous book than the Beth-Shan books, but even in this case we wonder how the money expended on the work compares with that expended on the publication.

The excavation of the *tell* at Beisan was started by Dr Fisher in 1921 on behalf of the Museum of the University of Pennsylvania; in 1925 Mr Alan Rowe, the author of the first volume, took over the work as Field Director, and he was succeeded in turn last year by Mr G. M. FitzGerald, who is the author of the second volume. In spite of these changes and though work has been suspended for a year or two more than once, there has been no serious breach in the continuity of the supervision, for Mr Rowe and Mr FitzGerald were members of Dr Fisher's staff and Mr FitzGerald remained with Mr Rowe after Dr Fisher had left for Megiddo.

At present Beisan is an insignificant little town: the railway from Haifa to Damascus passes through it, but the trade with Damascus is no longer of much consequence and the important through lines of communication in Palestine are those which run north and south. In old days it was not so, and from the second millennium B.C. down to the Middle Ages Beisan, which commands the Jordan end of the easiest east-west route through Palestine, was a great centre by virtue of its position and the fertility of the surrounding country.

Mr Rowe's volume includes a full account of the different levels on the *tell* and a more summary account of the temple found there with the very important hieroglyphic inscriptions belonging to them, and a most useful collection of the literary references to the place. It is illustrated with more than fifty plates which are the work of the Boston Heliotype Co.

The greater part of the Roman and later town lies on the hills round the *tell* but the *tell* itself, which was the early centre, was occupied down to the Arab period and the upper strata are like the upper strata in many other Palestinian sites. The use of cut stone introduced on a large scale first perhaps in Hellenistic times provided a material which could be re-employed again and again, with the result that clean sweeps were made of whole series of buildings, and the deep foundations which were laid here as elsewhere in the late Roman and Byzantine periods worked further havoc with these

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strata. It is only when one gets back to the earlier period when mud bricks were in use and the comfortably familiar processes of what has been called *tellification* began to operate, that we reach a series of well-marked levels with reasonably substantial strata between. According to the schematic diagram on plate I there are five levels between 1501 and 1225 B.C. and in less than three hundred years the *tell* rose some five metres in height, whereas in the very much longer period since 301 B.C. it has only risen half this amount at the highest point. In the earlier strata the most important finds are the great series of superimposed Canaanite temples which Mr Rowe has reconstructed with extraordinary ingenuity, a fine Assyro-Babylonian relief in two registers each with a lion and a dog, the stele of Mekal, those of Seti I, inscribed lintels of the time of Ramses II, a stele of the same kind and other inscribed fragments, the whole forming a wonderful series of monuments especially in a country so poor in inscribed material as Palestine. Illustrations of these finds are published in this volume but Mr Rowe promises us a more detailed account in the final publication of the Four Canaanite Temples which is reserved for volume II of this series.

Mr FitzGerald's volume forms the second part of this volume II and it is a masterly account, with excellent line illustrations, of all the pottery found on the *tell* except the cult objects. The account in fact is so good that we cannot refrain from expressing our regret that the pottery from the graves has not been published in the same volume.

The third volume of *Beth-Shan Excavations* deals with the Arab and Byzantine antiquities which were found on the summit during the seasons 1921-1923. This book, like volume II B, is by Mr FitzGerald and the work could not have been placed in more competent hands, though Mr FitzGerald was only at Beisan for a short portion of the period in question. Antiquities of this date are so often treated by archaeologists in a cavalier fashion that this careful and lucid account of both buildings and objects is doubly welcome.

It will not surprise those who are familiar with Palestinian sites to read that, owing to continuous occupation, very little of the buildings remained and it was impossible to ascertain the original ground plan of a single Byzantine house. Two buildings of some interest were however found, a gate flanked with towers at the northwest corner of the *tell* and a remarkable circular construction on a higher level. To the latter, which Mr FitzGerald identifies as a church, two chapters are devoted. The building in question consisted of a circular colonnade or *stoa* with an entrance on the west side and an apsidal extension on the east. Hardly anything was left above ground level and the reconstruction of the original structure is a matter of some difficulty. The editor argues with reason that the central area was never roofed: the width of the space to be spanned—more than 25 metres, the slightness of the surviving columns and the wall on which they rested, and the existence of a drainage gutter in the southwest section of the colonnade, are proofs in point. This being the case, however, are we justified in calling the whole construction a church? Admitting that the apsidal extension at the east may be a chapel, we should prefer to describe the colonnade in front of it as a circular atrium: it is none the less interesting on that account. It is lamentable that none of the fittings of this apsidal extension were found: if it was a chapel arranged like those at Jerash, the altar would have stood on the chord of the apse and there would not have been more than one step between it and the part further west, which would have been a *solea*. The colonnade was destroyed before A.D. 806 and not a single column base was found in position. Mr FitzGerald argues that there were only six columns on each side,

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but the intercolumniation which this number gives (4.06 m.) is very wide even for a wooden entablature, and we are inclined to assume double the number of columns. The geometrical principles on which the plan was set out are convincingly explained in the second chapter on this building, which should be studied by all who are interested in the development of East Christian architecture.

Of the objects found the most interesting are a fine series of bronze polycandela, a bronze lamp standard, a bronze censer, a stone mould for baking Eucharistic bread, and the incised and painted Arab ware shown on plates xxvi and xxvii. The inscriptions in Greek, Latin, Arabic and Hebrew, are adequately published, and the volume forms a notable addition to our knowledge of the period.

The title of the fourth volume recalls Josephus' account of the siege of Jerusalem : Josephus says that there were three walls on the north side of the city, that the third and most northerly of these was begun by Herod Agrippa, abandoned by him from fear of displeasing Claudius, and finally completed in great haste after the outbreak of the 'first revolt'. The line of this wall has been the subject of an old controversy : some identified it with the existing north city wall, others with the remains further north which were described by old travellers such as Pococke and Robinson but have since disappeared.

This controversy was revived by a chance discovery made in 1925 close to the site of the present Government Museum, and the excavations described in this volume were undertaken in the hope of settling the question finally. A series of modest trenches brought to light a line of walling extending over a distance of more than 500 metres : in one place the wall was traced for a continuous length of 81 metres and its general character was clearly established. It was from 4 to 4½ metres wide and defended by towers ; there was a gate just west of the American school ; the masonry was curiously irregular : a bedding of small stones lay between the rock and the first course which was the only one of which much remained ; in this course some stones were of great size and admirably dressed, one of them measuring over five metres long, others were of an inferior character. An attempt was also made near Herod's Gate to find where this wall cut the present north wall but the excavators were unfortunately compelled to abandon this before any conclusive result was reached.

The work actually accomplished is very clearly described, the relevant literature is quoted and an excellent series of photographs and plans are provided : the reader therefore has all the material available to appreciate the bearing of the new finds. To the present writer it seems that Dr Sukenik and Dr Mayer have made out a strong *prima facie* case for their hypothesis ; and they have shown that this wall occupied an excellent strategic line well calculated to defend the northern suburb of the Herodian city. The masonry, it is true, is too irregular in character to be identified as the work of Agrippa but, as the writers argue, it is uncertain how much Agrippa really completed—in parts the wall may have been almost finished, in parts only the foundations or not even these may have been laid—and in any case it is clear from what happened in the times of Florus and Cestius that as a whole this wall presented no serious obstacle until after the outbreak of the 'first revolt'. The character of the walling actually found is consistent with our writers' suggestion that this section at least was hastily assembled to meet a desperate emergency, like the Third Wall of Josephus. It is much to be hoped that Dr Sukenik and Dr Mayer may be able some day to close the question finally by completing their investigations near the existing north wall.

J. W. CROWFOOT.

ANTIQUITY

HISTORY OF PALESTINE AND SYRIA TO THE MACEDONIAN CONQUEST.

By A. T. OLMSTEAD, *Professor of Oriental History, University of Chicago*. Charles Scribner's Sons, 1931. pp. xxxiii, 664, with 188 illustrations, 18 plans, 1 map. 30s.

The historian of ancient Palestine and Syria is faced, at the outset, by one almost insuperable difficulty. His province, thus geographically divided, falls into two sections widely divergent both as to extent and as to international significance. Syria, if with Prof. Olmstead we prolong it as far to the north as Marash, not only contains an area more than three times as large as that of Palestine but completely outvies its smaller neighbour in the size, the number, and the importance of its ancient sites. There is, in fact, no comparison between the two countries when we estimate the parts played by them in the secular history of the Near East. But when the historian turns to the documents which must provide him with his material, he has to deal with a situation in which these rôles are completely reversed. Of the great civilizations of Carchemish and Kadesh-on-the-Orontes, of the brilliant coastlife of Byblos, Tyre, Simyra, and a dozen other seaports, of the rise and fall of Amorite, Hittite, and Aramaean dynasties he can win no more than a series of brief and intermittent glimpses, whether from the disinterment of their ruins or from the terse records of their successive conquerors. It is only when he is considering the vicissitudes of Israel and Judah that he finds not merely a sufficiency but even an embarrassing wealth of documentary evidence. And here further difficulties confront him. He has to face, first, the task of secularizing what is for many of his readers a Sacred Book and to determine how far its predominant interest in religious problems is to be treated as relevant and essential. He has to decide, secondly, how best to exercise a wise economy in re-telling a story at once familiar and profuse in detail. Finally, on a general survey of the material at his disposal, he must make choice between the possible alternatives, whether to accept the history of Israel as the central interest of his work, to set it boldly in the fore-front of the larger canvas, or to allow it to take its place—and that of necessity a small one—in the crowded panorama of contending empires. The mere statement of these multifarious problems is enough to indicate the magnitude of the historian's task, and it is no carping belittlement of Prof. Olmstead's achievement to question whether he has satisfactorily solved them. The book he has given us seems curiously unbalanced and indecisive. If his purpose was to present an objective picture of the ancient civilizations of Syria (using that term now in its widest sense), he must incur the charge of having greatly overloaded his book with Old Testament matter. Details of such minuteness as the wise-woman's cry at Abel (to take merely one instance at random) are surely out of place in the survey of so wide a field, and a very large amount of the space devoted to prophetic utterances could, on this view, have been equally well spared. If, on the other hand, Prof. Olmstead's aim was to emphasize the ethical and religious superiority of Israel in contrast to the paucity of its resources and the greater brilliance of the nations surrounding it, it can only be said that he does his best to conceal this. We receive, rather, the impression of a small people struggling like its neighbours for greatness and only differing from them in being continually thwarted by its own religious leaders; and we feel inclined to ask whether these domestic conflicts deserve to bulk so largely in a history of the ancient Near East. The author does not, in fact, seem to justify on either view his copious reproduction of the Biblical narrative. One point of historical criticism must be mentioned. In dealing with the period of the Judges, Prof. Olmstead conceives himself at liberty to 'adjust' the various episodes without reference to their present

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position in the text. Curiously enough Prof. Garstang in his recently published work on this same period has demonstrated with great plausibility the essential integrity of the narrative and its remarkable corroboration by the contemporary Egyptian records ! So complete a contradiction in theory as this will make the reader wary of accepting too confidently the critical conclusions assumed rather than advanced throughout the present work. For the student, indeed, its value will very largely reside in the mass of new evidence, archaeological and epigraphic, with which recent excavations have provided us. On this score alone the book will be for long indispensable, for Prof. Olmstead has spared no pains in collecting and presenting the latest information from these sources. His illustrations are both numerous and excellent, though we should have been grateful for more (and better) maps, especially in those chapters which concern the geography and history of Syria. A few misprints have been noted : p. 119 'course' for 'coarse' ; p. 336 (last line) 'beings' for 'begins' ; p. 434 'Quirl' for 'Quril'. The description of the vases in fig. 121 is misleading, for only three of these can correctly be termed 'Philistine pottery'.

W. J. PHYTHIAN-ADAMS.

THE QUARTERLY OF THE DEPARTMENT OF ANTIQUITIES IN PALESTINE. Vol. 1, nos. 1 and 2. *Jerusalem : Published for the Government of Palestine by Humphrey Milford, Oxford University Press, 1931. 5s each, yearly 18s 6d post free.*

The appearance of this new periodical will be greeted with satisfaction by archaeologists, though the fact that it is due to the munificence of Mr J. D. Rockefeller, Jr., will be viewed by those of them who are English with mixed feelings. 'Its main purpose', to quote the introduction by the Director of Antiquities, 'is to publish (a) any discoveries resulting either from excavations carried out by the Department, or from other methods of research, or that come to light in a more accidental manner in the course of the Department's ordinary administrative work ; (b) notes upon such antiquities in the Department's Museum, or elsewhere in Palestine, as have not already been published ; (c) texts and translations of texts describing historic monuments and sites ; (d) general news of archaeological work in Palestine'. The present numbers, accordingly, contain notes on a cemetery at Karm Al-Shaikh, Jerusalem, on recently discovered hoards of Phoenician and Byzantine coins, on a rock-cut tomb at Nazareth, and on a selection of coins in the Palestine Museum. To these are added articles on the medieval Ajlûn and the medieval Arabic description of the Haram of Jerusalem, and a collection of Arabic inscriptions 'unpublished and inadequately published' of which the Department possesses squeezes and photographs. Pre-Roman archaeology is represented by a 'Concise Bibliography of Excavations in Palestine', which, when completed, will be extremely useful to scholars. It will be seen from the above that so far the Arabist and the Numismatist divide the feast pretty equally between them, for the few crumbs of Roman and Byzantine glass and pottery offered will not go far towards assuaging the appetite of Stone-, Bronze-, and Iron- Age students. It is to be hoped that in future numbers the Department will redress these proportions so as at least to preserve a balance of interests. Meanwhile we extend a hearty welcome to the new venture, which is beautifully illustrated and produced in a style worthy of its future importance. The incorrect plate-references which will be noted in no. 1 pp. 3-5 are corrected in an erratum inserted in the second number.

W. J. PHYTHIAN-ADAMS.

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Roman Gaul

We have received from Professor Grenier a rejoinder to the review published in ANTIQUITY for September 1931 under the title of 'Roman Gaul'. With his assent this has been translated by the Editor, and submitted to Mr Ian A. Richmond, the writer of the review, whose reply to Professor Grenier is appended so that both may be read together.

The article 'Roman Gaul: a Review' (ANTIQUITY v, Sept. 1931, 344-350) contains numerous mistakes. It was not at Vienne that there were found sculptures parallel to those of St. Remy, but on the triumphal arch of Orange. There is no Roman crenellation at Saintes. Neither have I seen any at Fréjus near the Porte des Gaules, and I have never penetrated into the Villa Léoncine whose owner does not like archaeologists. But I have had the facts verified; and I am told that there is nothing. Doubtless that is why there has been no reference to them. The author of the review is equally unfortunate in his denial of the existence of the *castellum* of Senon, near Verdun. There can be no doubt whatever about it. I have seen it; Liénard and Chenet have excavated there, and also Drexel, during the War, in the course of his duties as Gefreite of the Landwehr.

It would be easy to quote other instances of statements or denials equally rash. The criticisms are no surer.

The *titulus* is said, on p. 346, to be a 'distinctive test for Roman work'. By *titulus* is meant either a ditch (*e.g.*, as at Heddernheim) or a series of small parallel ditches (*e.g.*, as at Xanten), placed in front of the gate of the camps. But such works are often absent. They are, in fact, an additional defence, disposed according to circumstances; in cases of siege they were as awkward for the defence as for the attack, for they prevented a sortie being made. The reviewer says: 'The omission of the fine long one at Urmitz is not excusable today'. That certainly is not the opinion of M. Lehner of Bonn, who has proved that this great ditch of Urmitz belongs to the neolithic earthwork (*enceinte*), in an angle of which the Roman camps are situated. It would have been wiser, after such a blunder, not to speak of Glozelianism.

It is proper to state that similar works, in Arabia and in the west, are not necessarily contemporary. There is no object in comparing works which differ both in date and character, *e.g.*, Eysses and Cirencester; Nérès and Inchtuthill or Haltern. Of Eysses nothing is known except that there have been found there some inscriptions of auxiliaries, probably of the beginning of the 1st century. Cirencester is a town where a good deal of digging has been done, and whose period of prosperity dates from the 2nd century. Nérès was a watering-place; Inchtuthill is a camp where, as often elsewhere, a military bath-house has been found. There are traces at Nérès of two camps, one of which looks like a prehistoric promontory fort, whilst the other (much smaller) remains undated. Neither of them has the slightest resemblance to the two Augustan camps of Haltern.

To understand the plan of the 1st century Gallo-Roman towns and the course of their enclosing walls (*enceinte*) one must bear in mind the distinction, classic since Mommsen, between Roman and Latin *coloniae*. The former were for the most part colonies of veterans; they preserve their wholly military character (Tac., Ann. xiv, 27, 4). It is legitimate to speak of *castra* in connexion with them. But it is quite otherwise with Latin colonies, whose population consisted mainly of natives. Vienne, which no

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one has ever thought of comparing to a camp—Nîmes—resembles Autun which had not the name of colony; and all betray by their shape and plan certain points of resemblance to the one Celtic *oppida*. The towns of Roman Britain present moreover the same contrast.

It is true that we have no good archaeological plan of Nîmes later than the 18th century. The *Forma Orbis Romani*, directed by A. Blanchet, whose first map has just appeared,* will shortly provide one. No doubt we would gladly have details of that iron grill of the Porte des Eaux, found at Nîmes in the 16th century; but unfortunately it has since disappeared. But let us not revive defunct hypotheses in a still more improbable form. It is long since the attempt to make the Tourmagne into a signal-tower was given up. Let us not now make of it 'a great Pharos-like tower', to light up the waste land round Nîmes presumably. It is known that, inside the tower, the lower stages have completely disappeared. It is perhaps rash to conclude from their personal arrangement that the monument can only date from the time of Augustus, and to reject the results of the patient observations of Espérandieu and his predecessors; but this is what the reviewer does when he states that, in spite of the absence of any connecting link in the masonry, the Tower was designed to form part of the rampart.

So far as fortifications of the 3rd and 4th century are concerned, there are close resemblances between Gaul and Britain, in spite of their divergent history. But there is an element essential in making comparisons between the fortresses of the two provinces—the dimensions of those fortresses. One must not compare a fortified town (*castrum*) with a *castellum*, a mere roadside fort, posting station and *horreum*. There is no 'Rhine-land' type of fortification. On the banks of the Rhine we find the same plans and the same architecture as inland; and no one has ever questioned their military origin.

'Why Tournus should be compared with Alzei rather than with Andernach passes the comprehension' of the reviewer. A little closer attention would have enabled him to understand the point of this comparison—in which moreover there is nothing essential—and many other more important matters.

One of my friends, an architect, once declared, in front of some Roman ruins—'Ancient architecture is quickly seen'. It seems that confronted with Roman Gaul the reviewer is like the architect.

Reply by Mr IAN A. RICHMOND:—

I have read Professor Grenier's reply to my review with great interest, and grieve that he should have misunderstood some of the points raised by me with greater interest in the subject than he has been willing to concede.

The Vienne sculptures are, of course, nothing like those of St. Remy in subject, but that was not the point at issue. I was drawing attention to Gallic feeling which both sets of sculpture exhibit and of which the variety is illustrated by the very diversity of subject. Nor did I deny the existence of the structure at Senon. I doubted, and still do, whether it is essentially a fortress. Again, the Urmitz *titulus*, to which I called attention, appears on the official plan in Bonn Museum just outside the west gate of the smaller Roman camp, where it is omitted on Professor Grenier's plan; and anyhow, while he rightly observes that permanent works often lack *tituli*, the context in question

* Doubtless M. Grenier's statement is correct, but there is no knowledge of this map in Britain, nor has a copy been received by the translator in any of his various capacities.—O.G.S.C.

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dealt with *castra* and my contention was that these, axiomatically, should have either *claviculae* or *tituli*.

The parallels with which fault is found were mentioned to arouse the interest of fellow-readers, as they aroused mine, always with the assumption that Professor Grenier would know them. But it must now be mentioned that Eysses and Cirencester were both important road-junctions, and that they have both yielded auxiliaries' tombstones belonging to their earliest days, thus raising the same problem of interpretation of these relics. Again, the site of Nérès, with its promontory fort, its two Roman fortifications and its unattached barracks, is strikingly like Inchtuthill, where there are not only *two* bath-houses, but (a) a promontory fort (b) a large camp (c) a small camp and (d) barracks unattached to either (b) or (c). Surely, too, this is reminiscent of the tangle of *Uferkastellen* at Haltern.

The Tacitean citation refers to obsolete conditions which were once (*olim*) typical, no doubt under the Republic and in Cicero's day. But where is there in all Gaul a castrametated colony of this type? And how can the contrast between *coloniae romanae* and *latinae* be illustrated in Britain, where there is not one town of the latter class?

Passing to Nîmes, my observations upon the structure of the Tourmagne are based entirely upon the *visible* remains, which anyone can visit today and which cry out for adequate publication. I did not assign this monument exclusively to Augustus, but ventured to date it not earlier than his day. Finally, my equation with the Pharos was one of form and not of purpose, and, while its value may be disputed, its truth can hardly be denied.

In dealing with fortifications, I may refer to Blanchet (p. 260) for the parapet near the Porte des Gaules at Fréjus. It is still there, and if ever my critic penetrates the grounds of Villa Léoncine, he will not miss the other example, as his informant appears to have done. In the discrimination of fortresses Anthes's plans are my witness that the round-towered fortresses are typical in the 4th century Rhineland. Meanwhile, the most careful attention does not yet persuade me that regular Alzei is the pattern for irregular Tournus, whether in purpose or type; for the comparison offends against the canons now advanced by its advocate.

My concluding action, however, must be to thank my critic for his interesting discussion. The mischance that some of my points were insufficiently clear has been the happy opportunity of providing at least one reader with a delightful specimen of courtesy and erudition, *d'autrefois*.

IAN A. RICHMOND.

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APRIL, 1932. 2s. net.

[VOL. XVII, No. 65

HISTORY

THE QUARTERLY JOURNAL OF
THE HISTORICAL ASSOCIATION

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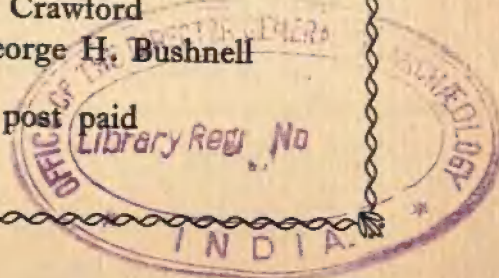
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Antiquity

A Quarterly Review of Archaeology

VOL. VI No. 22

JUNE 1932

Editorial Notes

ANTIQUITY has readers in every country in the World, from China to Peru, inclusive ; but the present number will be read perhaps for the first time by some who have come to London from distant lands to attend the International Congress of Prehistoric Archaeology ; and it is a suitable occasion therefore for setting down in plain words what ANTIQUITY stands for.



ANTIQUITY is primarily an archaeological journal, though we have always reserved the right to publish articles of a historical nature, if we think fit. We are, however, more closely concerned with prehistoric man and his environment ; within these wide limits we admit of no restrictions of time or space. We stand for the broadest conception of our subject ; we have attempted, often we know with success, to bridge the gulf between the specialist and the reader who is not a specialist in archaeological matters. (It is often forgotten that the specialist in one branch of science is usually a ' general reader ' in all such others as he may be interested in). During the 5½ years of our existence we have published such a wide range of articles that some of them are bound to be of interest to every reader of these words ; and if anyone decides, on reading, to become a subscriber here and now, as we hope, it is certain that he or she will not be disappointed so long as ANTIQUITY lasts.

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We believe most firmly in the need of studying man not in isolation but in relation to his environment ; that, for instance, you cannot understand the life of man in England during the Long Barrow (late Neolithic) period until first you have plotted his remains upon a map and then reconstructed, so far as possible, the vegetation by which he was surrounded. We believe in studying him on the sites where he dwelt, with fork and shovel (the old-fashioned method with pick and spade is now discouraged here) ; not merely in museums and libraries with note-books. We like also to show our readers what can be seen and discovered by air-photography. Finally, as good disciples of Tylor and Pitt-Rivers and their pioneer colleagues, we try to give life to dead bones by presenting pictures of primitive conditions today in out-of-the-way places. We could quote from back numbers of ANTIQUITY articles which illustrate each one of these principles.



But we cast our net wide in time as well as in space. In the present number we publish a Chronological Table of Prehistory covering the Old World from the Persian Gulf to the Atlantic and from the Arctic to the Sahara. Much labour has been spent upon it, yet still it is bound to fall short of the ideal. It will be superseded ; but it is, we know, a definite advance on anything of the kind yet published. It represents the considered opinion of scholars who command respect throughout the archaeological world. It does not pretend to take the place of the recognized text-books. It is not a text-book, but an indispensable supplement to such. It is intended to enable the reader to visualize the cultures or civilizations there mentioned in organic relationship with those which preceded and followed them. It is a pedigree of early human culture, as complete as can be in the present state of knowledge. Large regions of the world are necessarily omitted, because we still cannot correlate them with others ; but that is a defect of knowledge which is already being remedied in India, China and Africa.



This Chronological Table is an attempt, not the first of course, to visualize as a whole the main currents of human history. The geographical subdivisions adopted are necessarily, and for convenience, national ; but, as the Table itself shows, nations are ephemeral phenomena. It is a truism that Science is international ; it is one which

EDITORIAL NOTES

is particularly applicable to that branch of Science which is concerned with the origins of humanity. What does it matter to us whether a skull is found in England, France, or China? We are interested in Man, not merely in Englishmen or Chinamen. This universal aspect of Science is generally admitted, even by those who are blind to its political implications. It is, indeed, exemplified by scientific journals (including *ANTIQUITY*) which are read by students in every land, so far as the barrier of language permits.

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The basis of scientific organization is indeed becoming increasingly international. Archaeological books and articles in Scandinavia and the Far East are published in English, or with English summaries and underlines; German and French are used in Central Europe. Here, too, if we may be allowed to prolong the paean of self-praise (for which there is ample journalistic precedent), *ANTIQUITY* plays its part. Our contributors during the past years have been obtained from many different nations. In the present number we print, in English, a review of a book written in German by a Dutchman, contributed by a reviewer of Austrian birth holding an official position in Ireland. Archaeologically we have no racial or national prejudices; and we try to exclude them rigidly from our pages. We have only one standard—that of intrinsic scientific value.

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In recent numbers of *ANTIQUITY* there have been published several provocative criticisms of contemporary affairs. For some we are personally responsible; all are in our opinion 'fair comment upon matters of public interest' in the archaeological world. They certainly represent the private opinion of more than one qualified critic. But they are not the sort of fare usually served up at scientific meetings nowadays. In the good old days it was otherwise; now however there is a real danger of suffocation by platitudes. In a free-lance journal like *ANTIQUITY* such matters can be ventilated without malice and with more freedom than elsewhere; and if these occasional draughts of fresh air cause some people to catch cold we sincerely regret their inconvenience, but maintain that the atmosphere is improved, and the majority benefited thereby. Science has nothing to gain from polite humbug which no one really believes.

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The International Congress will begin in London on 1 August, and it will last until the 6th. Members will then disperse on excursions to all parts of the country for visits to places of archaeological interest. Membership of the Congress costs one pound and is open to all without restriction. The promoters hope that it will be well supported by all who are interested in prehistoric archaeology and they are anxious to receive applications for membership *now*: these should be sent to the Secretary, International Congress of Prehistoric Archaeology, Society of Antiquaries, Burlington House, Piccadilly, London, W. 1. We cordially support this invitation. The Congress is the first of a new series, and it rests with us in Great Britain to do everything we can to ensure its success.



In connexion with the Congress an exhibition illustrating recent field-work in Britain is being arranged at the London Museum, Lancaster House, St. James's. Included in this will be a display of air-photographs and maps organized by the Director-General of the Ordnance Survey in cooperation with the Keeper of the London Museum. The air-photographs have been enlarged to the maximum size possible and are from negatives made, in the ordinary routine of training, by officers and men of the Royal Air Force, together with some of those published in *Wessex from the Air*. A descriptive catalogue is being prepared for sale at the exhibition. The photographs themselves may be purchased by the public after the exhibition closes; but all rights of reproduction are reserved by the Crown. There will also be stalls for the display of archaeological books, pamphlets, periodicals and maps.



The maps displayed will show, in a concrete practical way, the great progress that has been made in England during the last decade in the geographical study of archaeology. They will indicate the present state of the great survey of megalithic monuments being carried out, sheet by sheet, by the Archaeological Department of the Ordnance Survey. For the benefit of our foreign guests there will be a selection of typical Ordnance Maps showing archaeological features. It is not generally known that Great Britain is the only country which marks all antiquities upon the published Government maps of all scales. The Congress and Exhibition should not be missed by readers of ANTIQUITY.

A Prehistoric Metropolis: the first Verulamium

by R. E. M. WHEELER

WESTWARD across the valley from St. Albans, the horizon is today fringed by a tract of secluded woodland. A part of this woodland has acquired the name of Prae Wood from a small medieval nunnery, dedicated to St. Mary de Pré, that once stood half-a-mile away beside the derelict Watling Street. The nunnery is now a patch of nettles in the meadows by the Ver, and the Street is a channel from which the metalling has been torn ; but both are better known to the historian and the antiquary than are the banks and ditches which lie hidden beneath the summer-jungle of Prae Wood. For all that, these banks and ditches are relics of a city which was at one time no less than the metropolis of a considerable part of Britain. It is nearly nineteen centuries since they passed into oblivion, and there is something of ironical irrelevance in the words of a Georgian milestone which chance has set up in their midst : ' 1 mile from Gorhambury, 20 miles from Hyde Park Corner '. The shade of Sir Thomas Browne might find a happy perch upon that milestone.

About the year 30 B.C., king Tasciovanus, suzerain (it would seem) of a large region in central and eastern Britain, struck coins which bear upon them, in one form or another, the name of Verulamium and are, incidentally, amongst the first inscribed coins minted in these islands. His predecessor, Cassivelaunus, had been the elected leader of the British tribes against Julius Caesar in 54 B.C.; and it is unnecessary here to discuss again in detail the plausible identification of Verulamium with the unnamed stronghold of Cassivelaunus which Caesar's legions stormed in that year. Suffice it to note that (1) the stronghold of Cassivelaunus, which was the Roman objective, may be presumed to have been identical with the headquarters of his successor, *i.e.*, Verulamium; (2) the position of Verulamium is consistent with the general line of Caesar's advance, and (3) the Prae Wood site, which recent excavation has shown to have been that of a late prehistoric city of considerable magnitude, might be adduced as an exact illustration of Caesar's commentary. His enemy's

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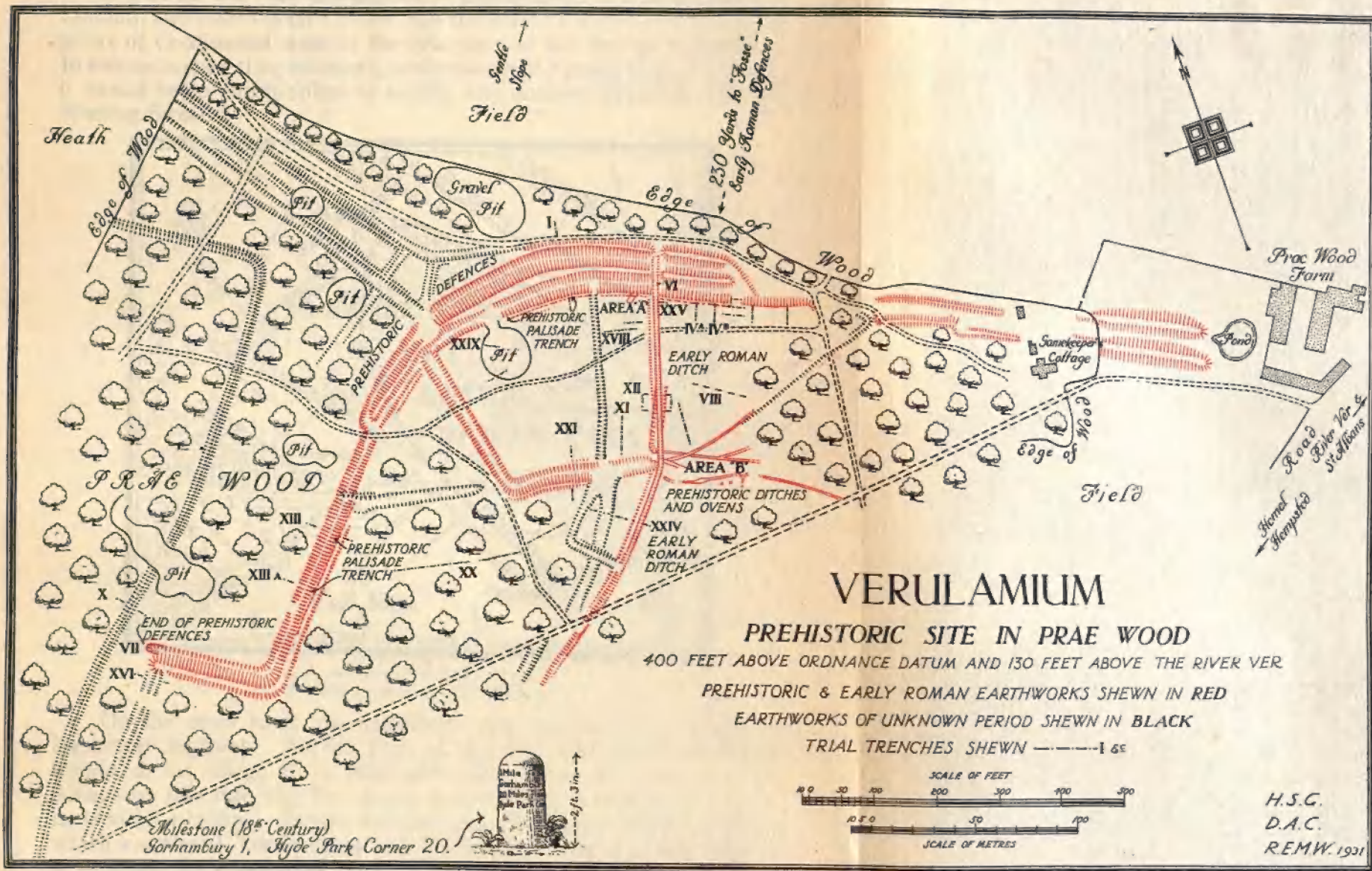
stronghold was, he tells us, 'protected by woods and marshes'; and he adds that 'the Britons apply the name of stronghold to any woodland spot, difficult of access and fortified with a rampart and trench, to which they are in the habit of resorting in order to escape a hostile raid'. Only on the slopes which lie between Prae Wood and the valley, 130 feet below, have the labours of Roman city-builders and post-Roman farmers cleared the woodland which would have completed the illustration.

It is the handiwork of these Roman citizens that has, until recently, focussed the attention of the antiquary. From the days of the Saxon abbots, who ransacked the 2nd-century Roman fortifications and the buildings within them for good pagan bricks, Verulamium has meant the great oval enclosure, 200 acres in extent, which lies along the lower slopes of the valley and is threaded by the Watling Street. When, therefore, in 1930 the systematic excavation of Verulamium was begun, it was in this area that the first trenches were cut. The absence of any evidence, in them, of prehistoric occupation within the southern half of the city led the excavators further afield, with the resultant discovery of earlier Roman fortifications of earthwork on the hillside to the west (fig. 1). A part of these earlier fortifications have long been known vaguely as 'The Fosse', but their historical significance had not been suspected, and does not, indeed, concern us here. It need merely be remarked in passing that they represent a 1st-century Roman Verulamium, smaller in size than the later city and fortified presumably as the result of the destruction of the unfenced town by Boudicca and her rebels in the year 61.

In the search for the prehistoric site, the discovery of the earlier Roman city on the upper slopes of the hill was a useful pointer. The next step was clearly to the hill-top itself where the earthworks already referred to, in Prae Wood, had been observed in March 1930 by Mr O. G. S. Crawford. There is no hint of them upon the Ordnance maps and the more important of them are here planned for the first time from a survey (plate 1) prepared mainly by Mr Huntley S. Gordon and Mr D. A. Casey. In 1931 Mr Casey, on behalf of the Verulamium Excavation Committee and with the ready permission of the Earl of Verulam, carried out a fruitful series of excavations here which finally settled the problem of the site of the prehistoric capital. The present summary merely anticipates Mr Casey's report.

In order to appreciate the topography of this site, it is desirable at the outset to bear in mind its *milieu* in the prehistoric era. In Roman as in modern times, a determining factor in the development of the

PLATE I





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A PREHISTORIC METROPOLIS

district was the great arterial Watling Street, running from southeast to northwest along the line of the river-valley. But the dominance of Watling Street was itself determined by the commercial pre-eminence of London, and commercial London was the product of the overwhelming influx of Continental trade in the first years of the Roman occupation. In reconstructing the prehistoric environment of Verulamium, therefore, it would be an anachronism to ascribe any decisive importance to the Watling Street.

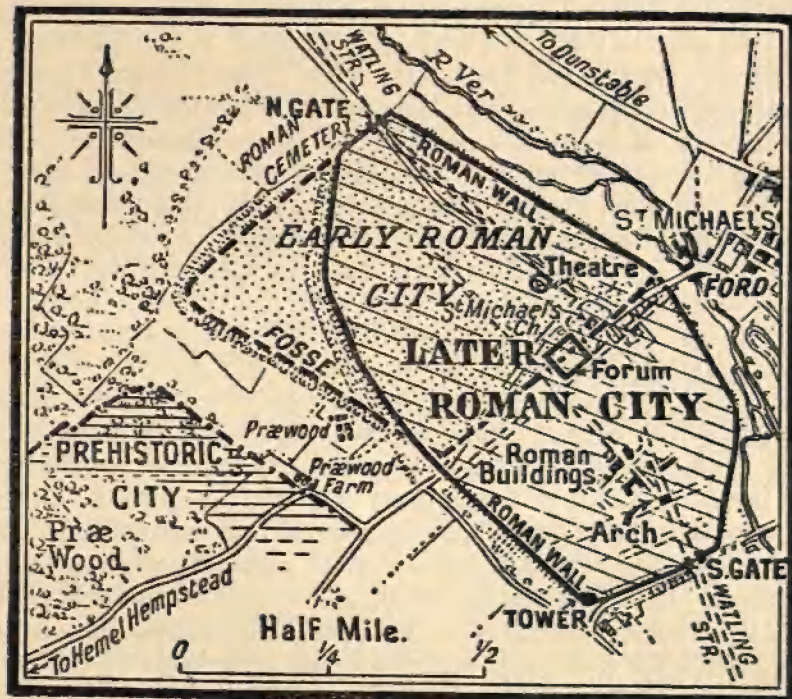


FIG. 1. Sketch-map of the three Verulamiums
(Reproduced by permission from *The Times*)

On the other hand, the prehistoric city commanded at least one important highway. At the foot of the hill whereon Prae Wood stands, in the village of St. Michael's which forms the outskirt of St. Albans on this side, the Ver rivulet is crossed by a ford, now supplemented by a bridge. Across this ford in ancient times passed the road which connected Verulamium and Colchester; and it is not difficult to estimate the importance of this highway during the last generations of prehistoric Britain when it be recalled that, at the beginning of the

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1st century A.D., under the impetus of a rudimentary but expanding Continental trade, the dynastic headquarters were moved by the famous Cymbelene, son of Tasciovanus, from the inland site of Verulamium to the more accessible site at the head of the Colne estuary. Indeed, at this period, the relationship between Verulamium and Colchester must in some sense have anticipated the later relationship between Roman Verulamium and London; and it may be said that, whilst the Roman Verulamium looked southwards to the Thames, its predecessor faced rather towards the east and the Essex coast.

If we now return to the surviving vestiges in Prae Wood, we are prepared, on these general grounds, to find that the main defences of the prehistoric capital line the north-eastward brow of the hill above the St. Michael's ford. The old trackway leading down to the ford may be supposed to have followed approximately the line of the present Bluehouse Hill (the Hemel Hempsted road). On the hill-top, this trackway may have formed the southern limit of the city; at any rate, all the surviving traces of earthwork lie on its more northerly flank, and continued search both on the ground and from the air has failed to discover any suggestion of bank or ditch to the south of it.

To the modern eye the earthworks of Prae Wood can only be described as a vast maze, consisting of banks and ditches of all sizes and shapes above ground and, as excavation has disclosed, a further series of ditches beneath the surface. They can, however, be roughly divided into two groups: defensive and non-defensive. The former begin today close to the buildings of Prae Wood Farm, but are shown, by an abrupt twist and sinkage of the adjacent stretch of the Hemel Hempsted road, to have continued at one time at least 200 yards further towards the southeast. To the northwest, the defensive banks proceed for nearly 400 yards and then bend slowly and in diminishing strength towards the southwest until, after a sharp outward turn, they disappear abruptly. At this point, the actual end of the ditch was uncovered during the excavations, thus showing that the earthwork in fact ceased here. Why it should have done so is not apparent, since the ground hereabouts is level and the subsoil is for some distance, both within and beyond the limit of the earthwork, a coarse gravel which implies no sudden phytographical change of the kind that frequently explains the disposition of primitive barriers. It may be supposed that the mixed but often dense woodland, which the variable subsoil of the site as a whole (ranging from a sandy clay to gravel) supported, made the construction of elaborate artificial defences less

A PREHISTORIC METROPOLIS

urgent on the south-western side, particularly since this part of the settlement lay furthest from the line of approach to the ford and, as excavation has indicated, did not contain the main areas of occupation. It may be observed, in this connexion, that the more westerly portion of the defences includes only a single ditch and is notably weaker than the multiple system which faces northwards and north-eastwards towards the valley. However irrational from a military standpoint, it would almost seem that the strength of the defences was proportionate to the intensity of the occupation immediately adjoining them: an inference which might suggest that they were thrown up at a time of crisis rather than as a part of the considered lay-out of a fenced town. It is tempting (but, of course, improper) to toy with the conjecture that these seemingly-incomplete defences were built hastily by the tribesmen of Cassivelaunus when Caesar's legions were already groping their way northwards across the Thames.

It may be that, towards the southwest, the woodland barrier was supplemented by a palisade. Certain it is that the earthwork defences were completed on their inner side by this device. At numerous points, the trench in which the stockade had stood was revealed along the lip of the ditch (plate III) or, where the system is multiple, of the inner ditch. Behind the palisade had been heaped a certain amount of earth from the ditch, but the palisade-bank seems never to have been of any considerable height. Palisades of this type were probably a common feature of the defences of our later prehistoric towns—for example, similar traces of them have been found at Hembury Fort in Devonshire, and at Cissbury in Sussex. The Verulamium palisade was built into an earlier 'occupation-layer' which included small fragments of pottery similar in fabric to the coarser wares found elsewhere on the site but for the most part of indeterminate form. At one point, the palisade-trench had been cut through and the palisade-bank had been built over a cobbled area, some 20 feet long and 9 feet broad (plate IV).

Where the surviving defences are strongest, *i.e.*, at the northern bend (plate II), they consist of the palisade and bank, an inner ditch, a median bank, an outer ditch, and a third bank with a final escarpment, and are 80 feet across from palisade to outermost bank. The median and outermost banks contained stray scraps of pottery similar to those noted above.

Within the area covered by the defences, numerous trial-trenches cut in the south-western quarter of the site yielded hardly any traces

VERULAMIUM THE PREHISTORIC SETTLEMENT IN PRAE WOOD AREA B

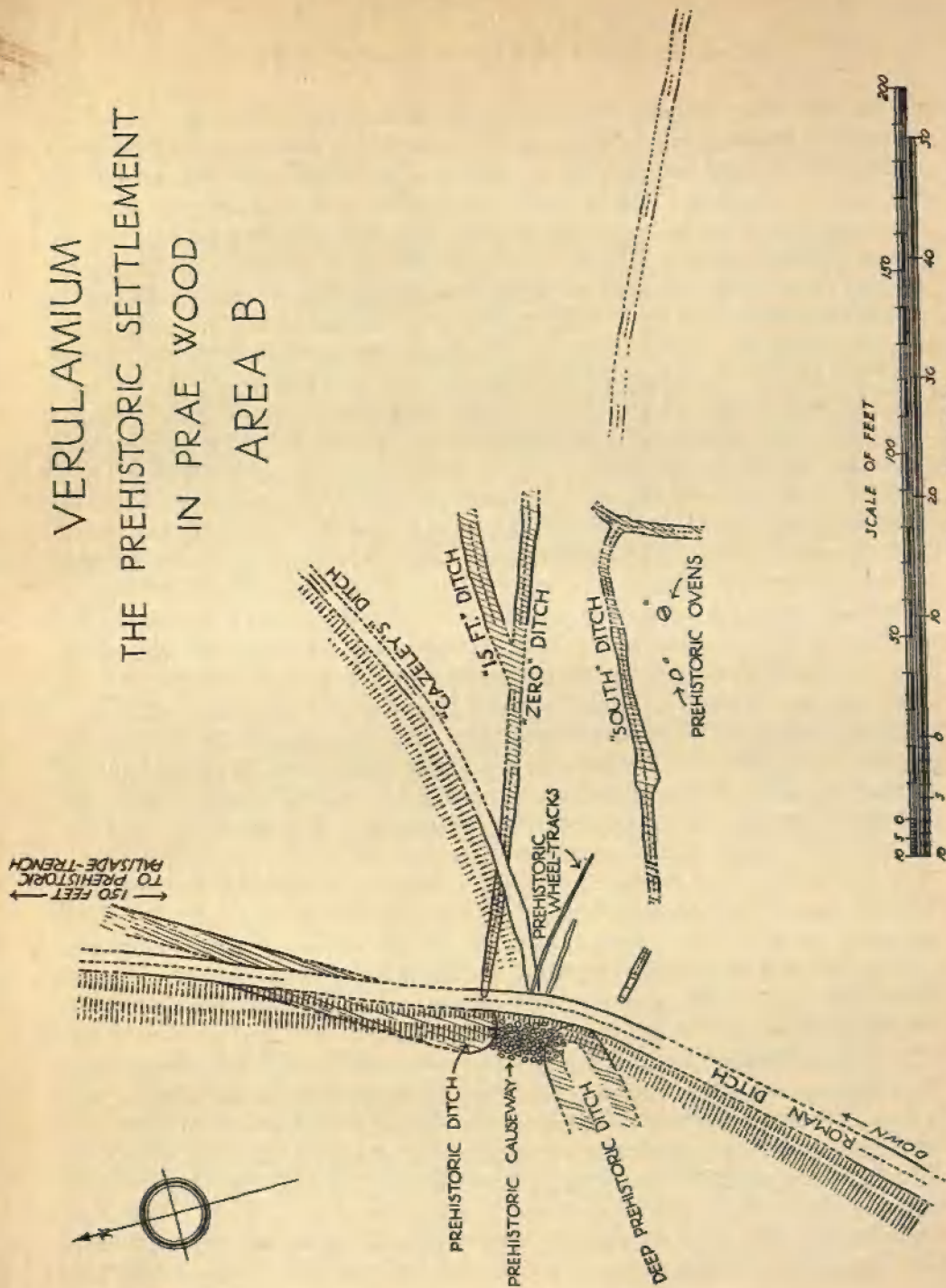


FIG. 3.

A PREHISTORIC METROPOLIS

of occupation. Where the defences are strongest, however, they shelter an enclosure of about $1\frac{3}{4}$ acres bounded by rectilinear lengths of ditch (see plan, plate I) some 7-12 feet wide and 4-8 feet deep, the upcast of which was, in part, thrown up on the outer side. These ditches had been filled up with debris (plate VI), including much ash and great quantities of pottery covering the period from the end of the 1st century B.C. to about A.D. 40. The filling was generally sealed by a layer containing fragments of Roman brick and Roman provincial pottery of the 1st century A.D., whilst the filling of the eastern ditch had been cut by a part of a subsequent drainage-trench also of the early Roman period (see fig. 2).

At the south-eastern corner of the enclosure, the prehistoric ditches stopped on each side of a causeway 14 feet broad, paved with a heavy layer of large flints. That this flint cobbling was an original feature of the work, was shown by the fact that the flints seeped down into the ends of the adjacent ditches and were there covered by the earliest prehistoric silting. Approaching this causeway from the south-east, two long ruts (see fig. 2 and plate V) were found beneath the surface, in the natural sandy clay of the site. Where best preserved they were parallel, 8 inches deep and 4 feet 7 inches to 4 feet 9 inches apart, centre to centre; and a few feet before the margin of the flint causeway was reached, at a point where the surface slightly dipped, the soil between them had, in ancient times, been churned up and softened, and had thereby attracted a forest of bracken roots. As they approached the causeway, the ruts had been cut by the early Roman drainage-ditch already mentioned, and were therefore prior to it. These ruts can claim the distinction of being the oldest wheel-tracks yet found in Britain, and may perhaps provide fresh evidence for the antiquity of the modern standard gauge of 4 feet $8\frac{1}{2}$ inches.

A clearance of the area in the vicinity of the wheel-tracks revealed an erratic system of relatively shallow ditches (see fig. 2), doubtless intended to drain the adjacent soil for habitation. The ditches were not all of identical date, but most of them had been used secondarily as rubbish-tips (plate VI) and contained pottery of the same late prehistoric phase. With the pottery were animal-bones and ash which had been tipped or swept into them, mostly from the southern side. A careful search failed to reveal any certain traces of hutments, but the remains of two ovens (plate VII), with slight traces of perhaps two more, were found near one of the ditches. These ovens had been oval on plan, with clay walls which had probably at one time been carried up

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to form a domed top. At one end of each oven, an extension of the burnt flooring indicated the position of the hob. Similar ovens have been uncovered on the foreshore of the Thames at Tilbury in Romano-British huts of native type, and were used probably for cooking food.

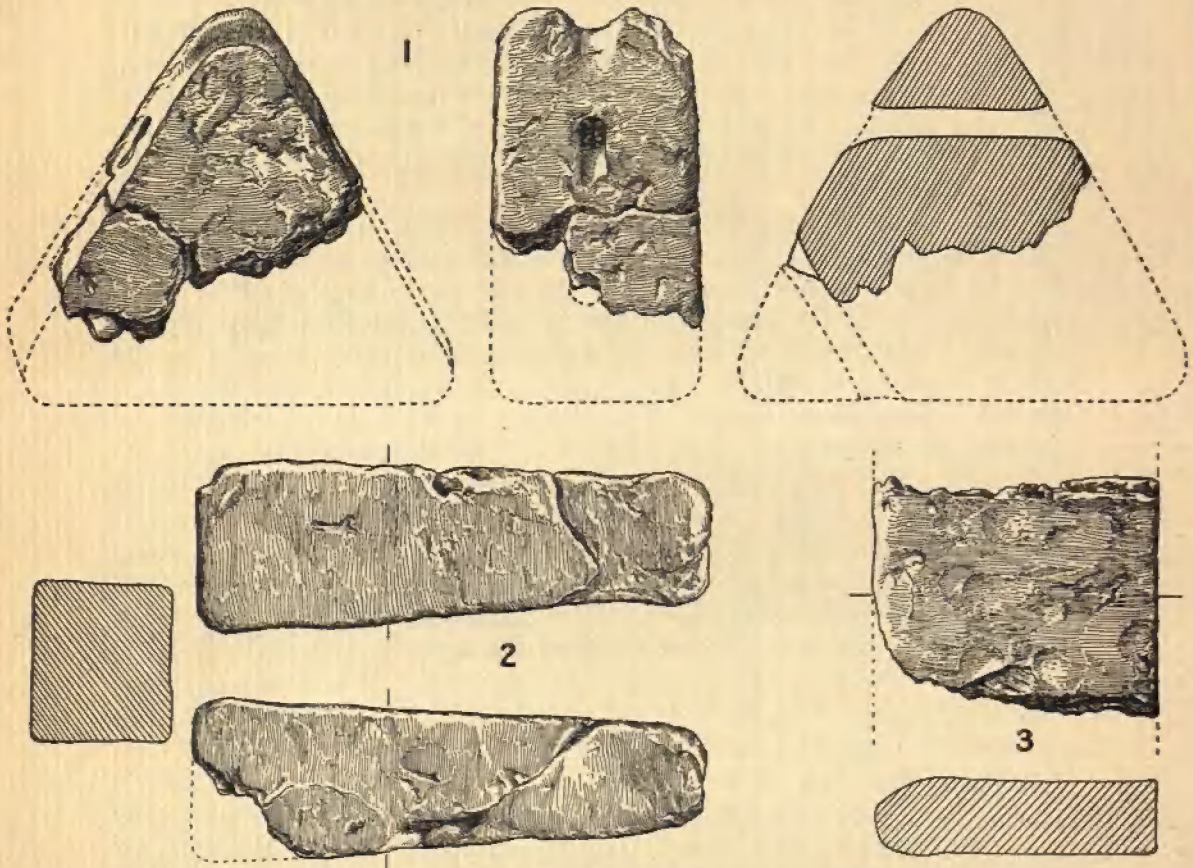


FIG. 3. 1, Clay loom-weight; 2, Clay fire-bar or pillar-support; 3, Brick

The Verulamium ovens had been floored with clay reinforced by potsherds, and contained fragments of pierced bricks which had presumably been raised on fire-bars to form a grid (see fig. 3, no. 2, and plate viiib).

Until further excavations have revealed remains of the actual dwellings of the prehistoric inhabitants, our knowledge of their environment remains inadequate. But a hint of interesting possibilities is

A PREHISTORIC METROPOLIS

given by the presence of considerable quantities of fragmentary brick among the debris in the drainage-trenches. These bricks (plate VIIIa and fig. 3, no. 3) are readily distinguished from those of Roman Britain. They are yellow or yellowish-red in colour and the clay is coarse and contains large quantities of pebbles and grit. Their thickness varies from 1 to $1\frac{3}{4}$ inches. Their surface is roughly kneaded into shape with

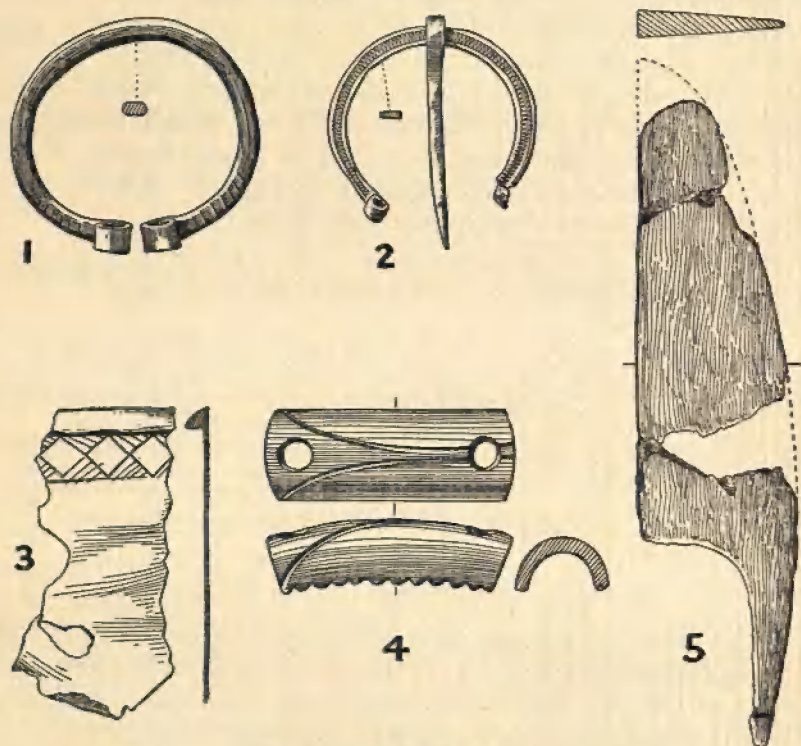


FIG. 4. Objects of metal $\frac{1}{2}$

the fingers, and at the edges they are normally rounded. No complete brick was found, but one had been $4\frac{1}{4}$ inches wide whilst others had been over 6 inches wide. No relic of a brick structure has yet been found, and there is no indication that lime-mortar was used. Brick structures were, however, built in some of the Gaulish *oppida* before the Roman conquest, and it may be that the Verulamium *oppidum* also contained houses built of these primitive bricks.

The general culture of the inhabitants showed something of that

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curious mixture of squalor and sophistication which we are accustomed to find in Early Iron Age Britain. Their crude bricks may be taken as a symptom of unusual enlightenment. The triangular loom-weights (fig. 3, no. 1) and clay spindle-whorls are evidence of a normal prehistoric home-industry of a kind to which similar loom-weights in the contemporary Glastonbury lake-village and elsewhere bear witness. Of metal-working, few traces have yet been found, but a broad-bladed knife (fig. 4) and a fragment of another are of common Iron Age form. No coins and few ornaments have so far come to light. Two bronze penannular brooches have their ends rolled forward, instead of laterally as at Glastonbury, and one of them is decorated with a roughly incised zig-zag pattern. A fragment of sheet bronze (possibly part of a sword-scabbard) is ornamented with incised lozenges. Again, the workmanship is of the rudest description (see fig. 4).

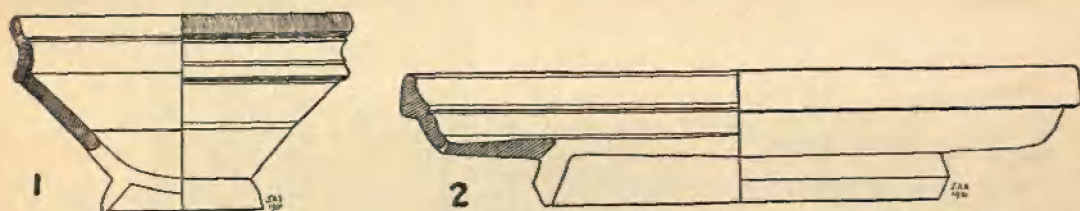


FIG. 5. Red-glazed (Arretine) ware, imported from Italy before the Roman conquest $\frac{1}{2}$

Of greater interest is the pottery, of which large quantities have been found. This may be divided into three categories : (1) imported vessels of Roman (mostly Italic) manufacture ; (2) imported vessels of northeast Gaulish or Belgic manufacture ; (3) coarse imitations of (1) and (2), together with other vessels of coarse fabric, most of which may probably be ascribed to local kilns.

A full description and discussion of these categories must await the preparation of the detailed report but samples of each category are here illustrated, with a brief commentary.

Category 1. Only two or three vessels of the red-glazed Italic or Arretine ware are represented. Two of them, which may have been made at any time between 20 B.C. and A.D. 40 are illustrated (fig. 5). Parts of a jug and a small amphora, both of classical type and dating from the first half of the 1st century A.D., were also found.

A PREHISTORIC METROPOLIS

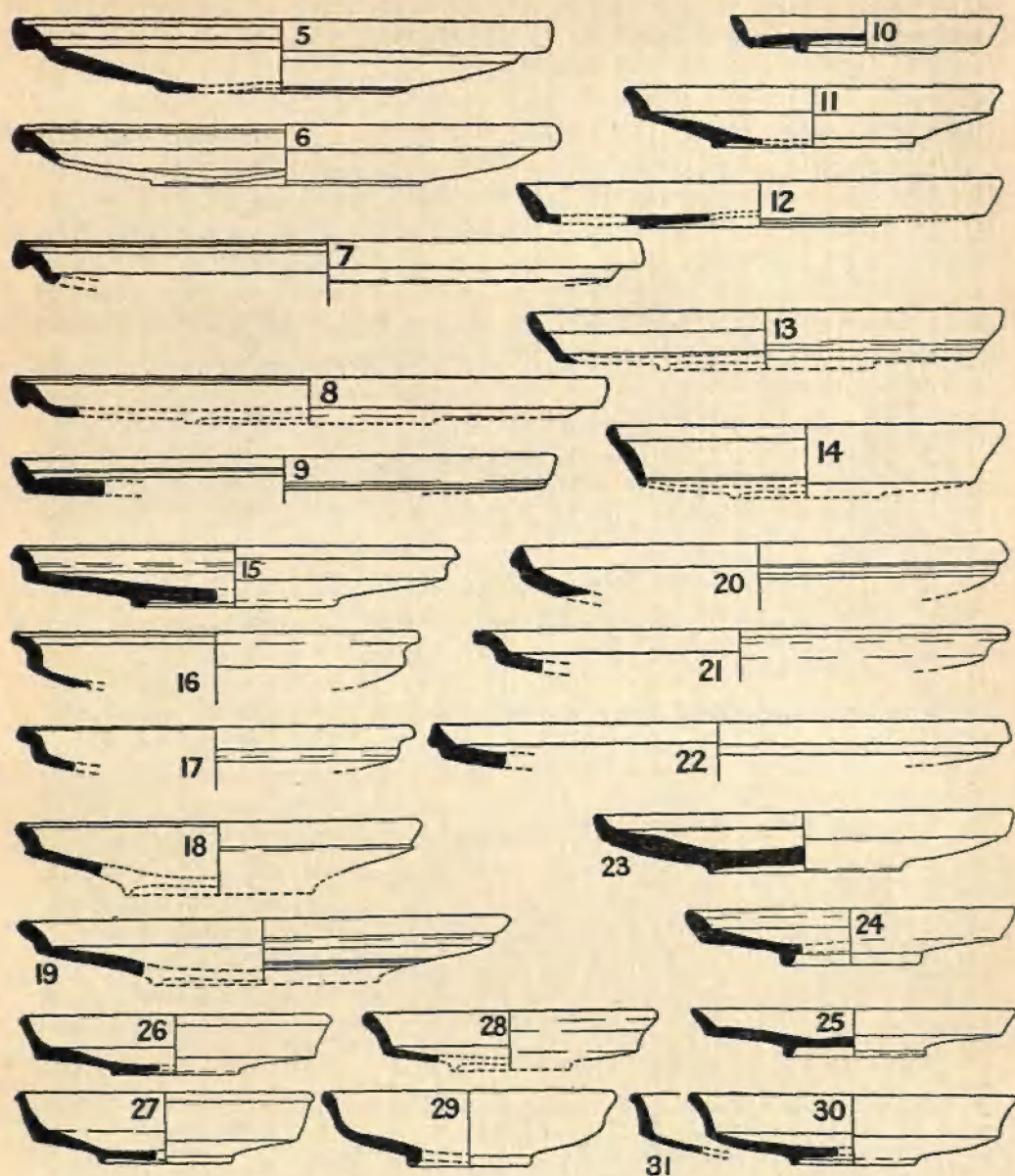


FIG. 6. Belgic (5-14) and sub-Belgic (15-31) plates copied directly or indirectly from Italian prototypes 1

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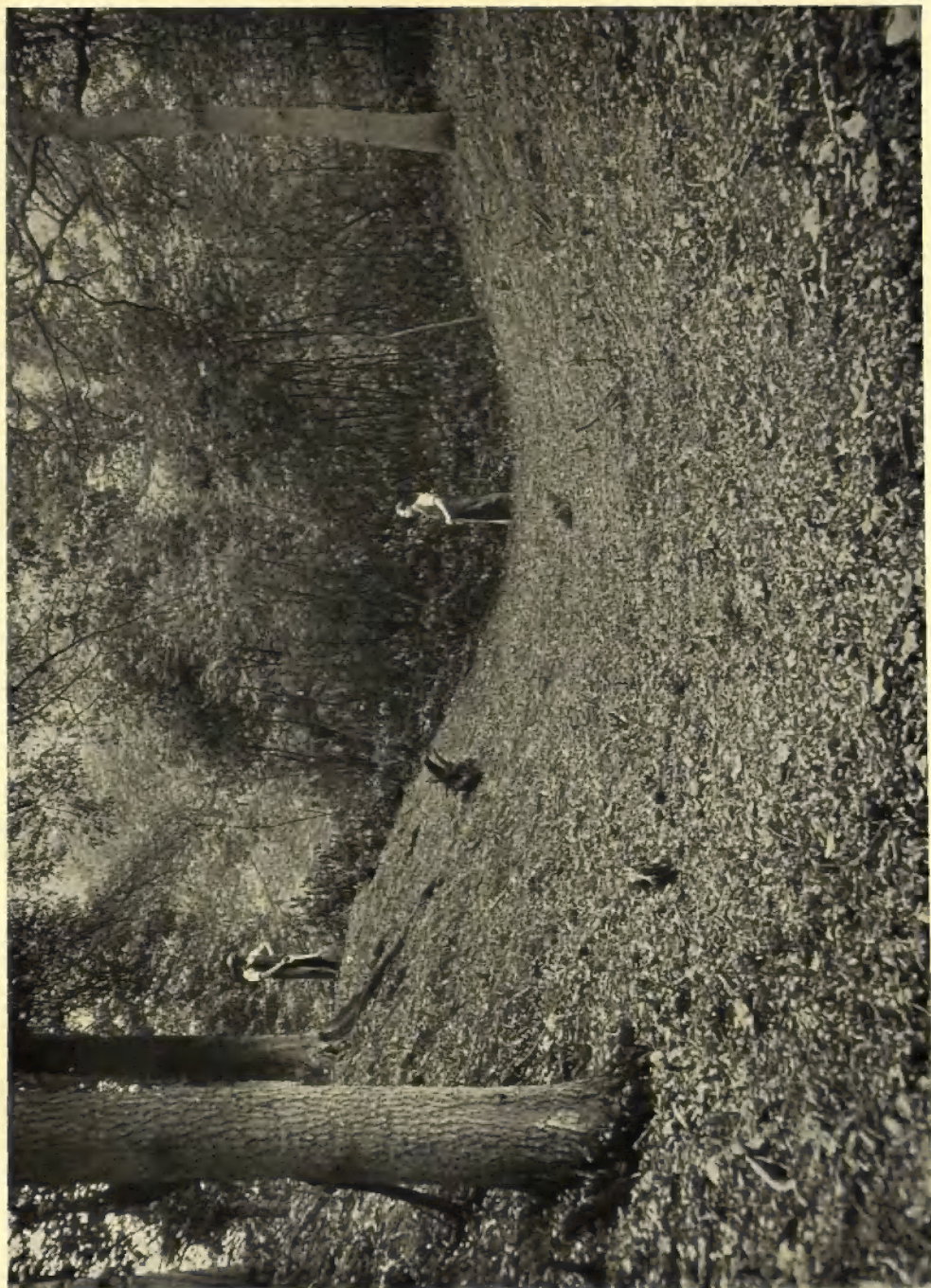
Category 2. Fragments of about 20 plates, of the hard, wheel-turned grey ware which the Belgic potters of the first half of the 1st century A.D. made in imitation of Arretine forms, were found and are sufficiently represented in fig. 6. These are not easy to date more precisely. One characteristically late feature of the Belgic type of plate, however, has not been recognized on this site: namely, the tendency of the foot-rings of the later examples to lose their function, and for the plate to rest instead upon its outer circumference. The functionless foot-ring does not appear on the Continent at Haltern (11 B.C. to A.D. 16) but is found at Claudian Hofheim (about A.D. 40 to 51). The absence of this late feature is one of many evidences that the filling of the Verulamium ditches was completed before the Claudian invasion of A.D. 43.

The following stamps of Belgic potters are represented:—TIOTAG (cf. *C.I.L.* XIII, pt. 3, fasc. I, p. 376, 1911); OTAV, or VATO retrograde, on fig. 6, no. 10; NONICO (cf. *C.I.L.* ib. p. 306, 1441); . . . AN, possibly BELAN, a stamp which occurs at Haltern; and IVF, perhaps an 'imitation' stamp, on sub-Belgic rather than true Belgic ware.

Category 3. Under this category are grouped the coarser wares, which may also in some cases be importations, although most of them are probably of more or less local origin. A majority of them are made on the wheel, but with very varying degrees of skill. They range from angular cordoned cups of excellent grey or orange-coloured clay (fig. 7, nos. 32-38), recalling, in form, the *tazzas* of well-known Belgic type, but generally with a simple base-ring rather than a pedestal; to large, roughly-scored vessels with heavy roll-rim (fig. 8, no. 46) of a type which, in this district, lasted occasionally into the 2nd century A.D. Plates which may be regarded as 'sub-Belgic', *i.e.* as local imitations of the Belgic series, are numerous (fig. 6, nos. 15-31), and occasional rough copies of other Italic or Arretine forms are also present (fig. 7, no. 45). The pedestal-foot which is usually regarded as typical of Belgic pottery occurs rarely (fig. 7, no. 32), only four examples all told. Butt-beakers, which are frequent on northeast Gaulish sites in the 1st centuries B.C.-A.D., are fairly numerous here also (fig. 7, nos. 43, 44). They are usually of orange-coloured ware, but occasionally of a hard white fabric which may be of Continental derivation.

If we summarize the evidence of the pottery, we may say that it has few links with pre-Belgic pottery in this country, and that, for the most part, it is, or is modelled upon, the La Tène III pottery of north-eastern France and the Rhineland, as supplemented and modified by

PLATE II



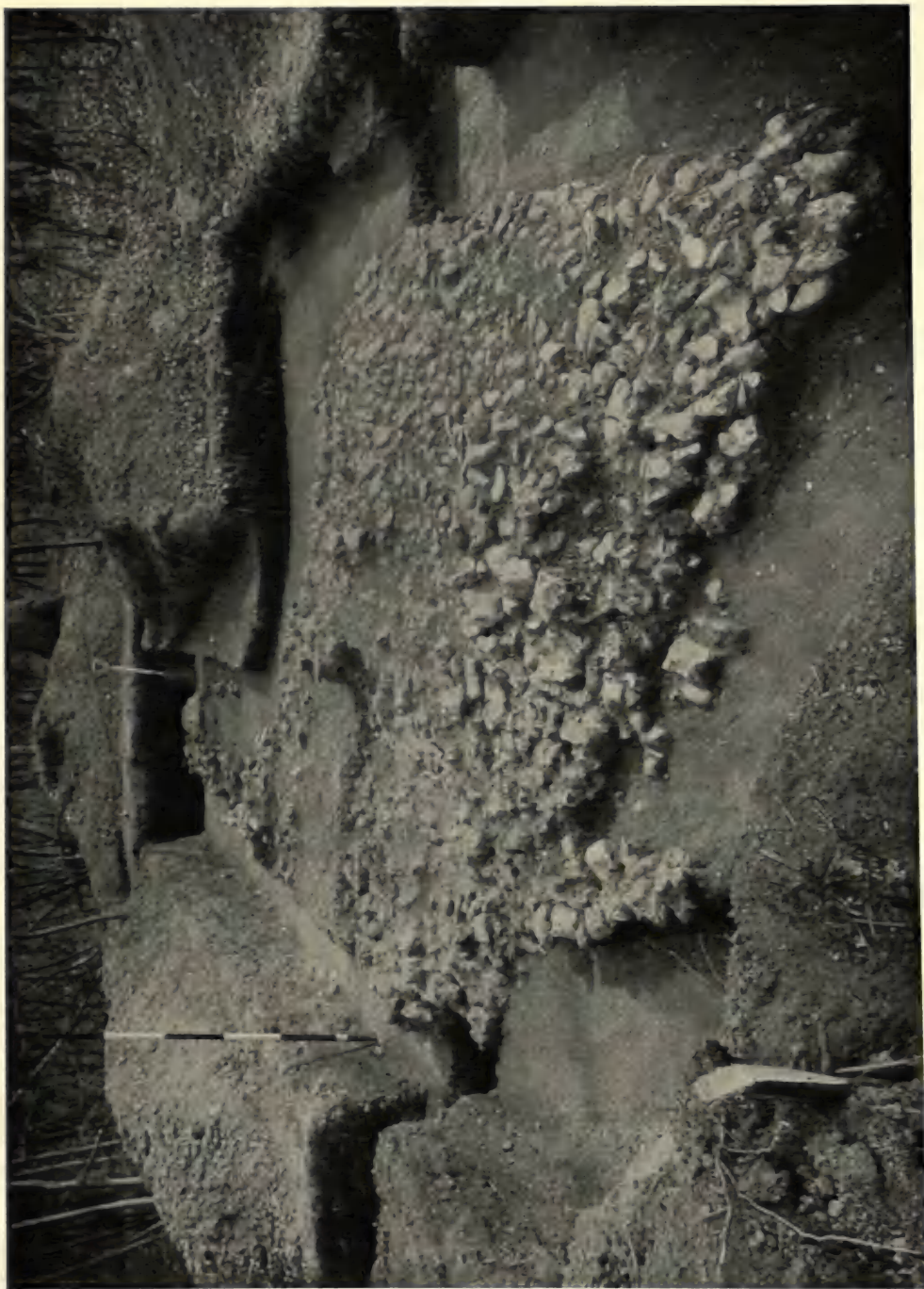
PREHISTORIC VERULAMIUM: INNER DEFENSIVE DITCH AT NORTHERN BEND, BEFORE EXCAVATION

PLATE III



DEFENSIVE DITCH (A) AND BANK WITH SLOT FOR PALISADE (B) ON THE NORTHWEST
SIDE OF THE SITE (XIII ON PLAN)

PLATE IV



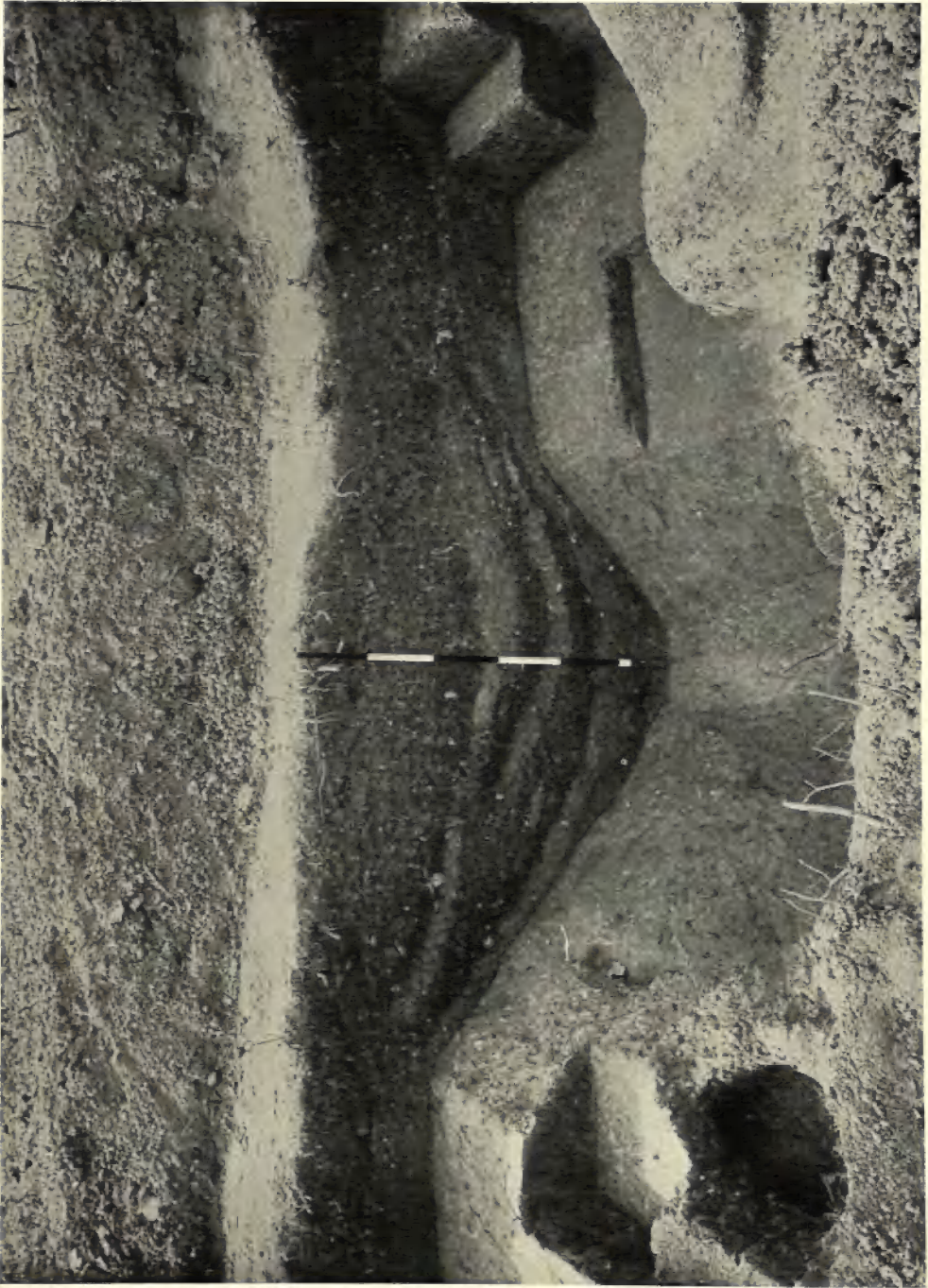
COBBLE FLOORING PRIOR TO DEFENCES, AT 'AREA A' ON PLAN. THE STRAIGHT EDGE OF THE FLOORING NEAR THE RANGING-POLE MARKS THE LINE OF THE PALISADE.

PLATE V



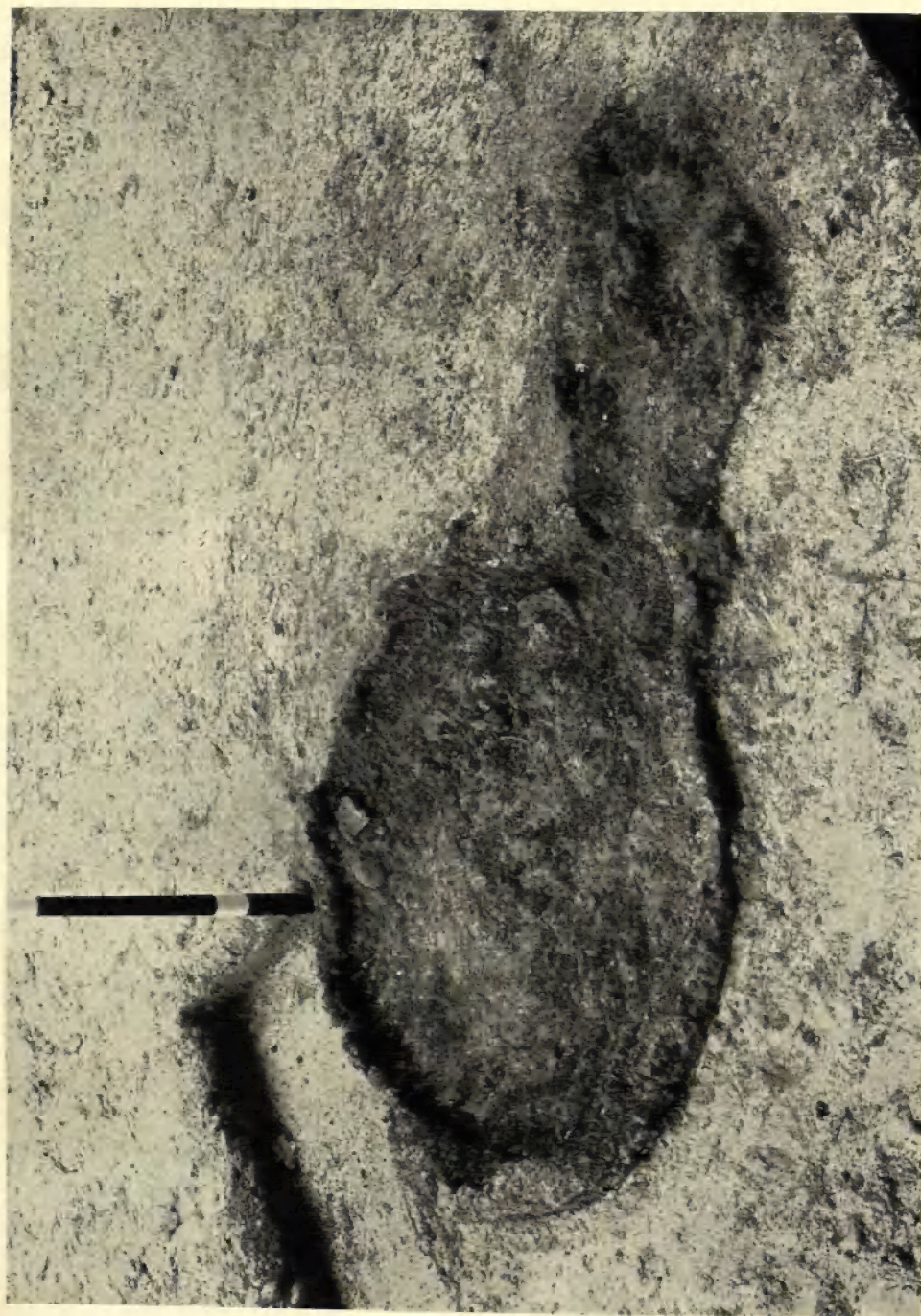
PREHISTORIC WHEEL-TRACKS, INTERRUPTED IN FOREGROUND BY EARLY ROMAN DITCH

PLATE VI



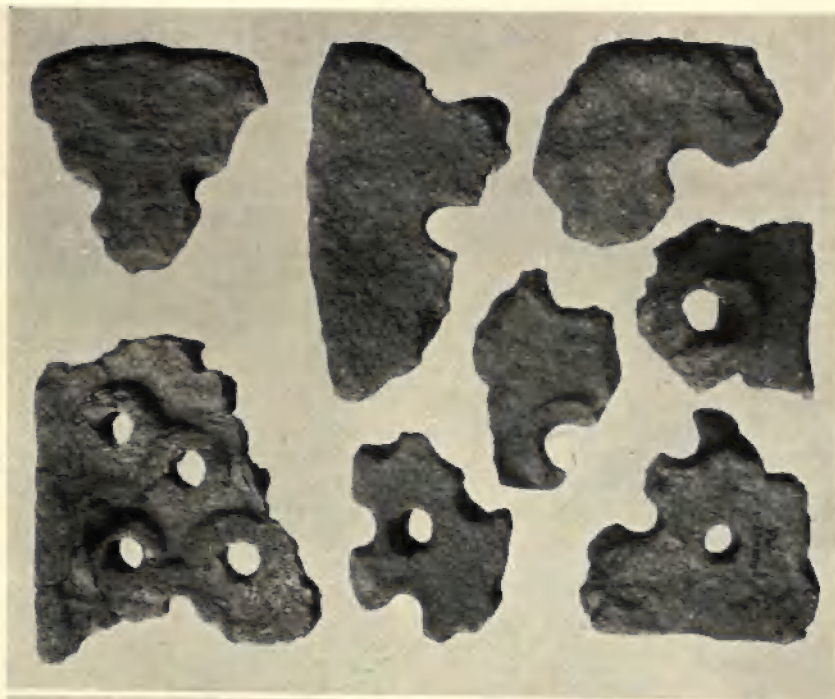
PREHISTORIC DITCH AT XII ON PLAN, SHOWING LAYERS OF DEBRIS-FILLING

PLATE VII

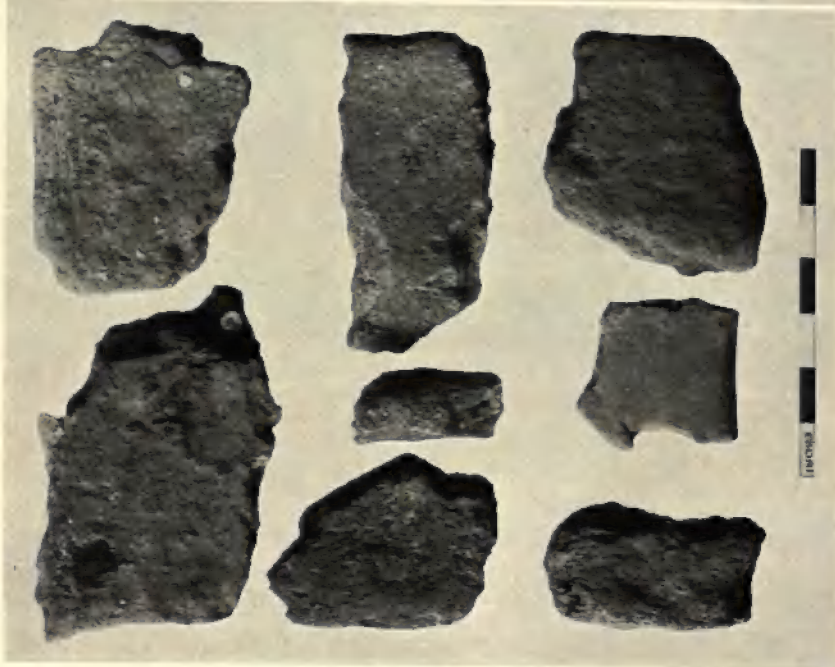


BASE OF PREHISTORIC OVEN

PLATE VIII



B. PREHISTORIC PIERCED BRICKS



A. FRAGMENTS OF PREHISTORIC BRICK

PLATE IX



POTTERY FROM PREHISTORIC VERULAMIUM
(scale of inches)

A PREHISTORIC METROPOLIS

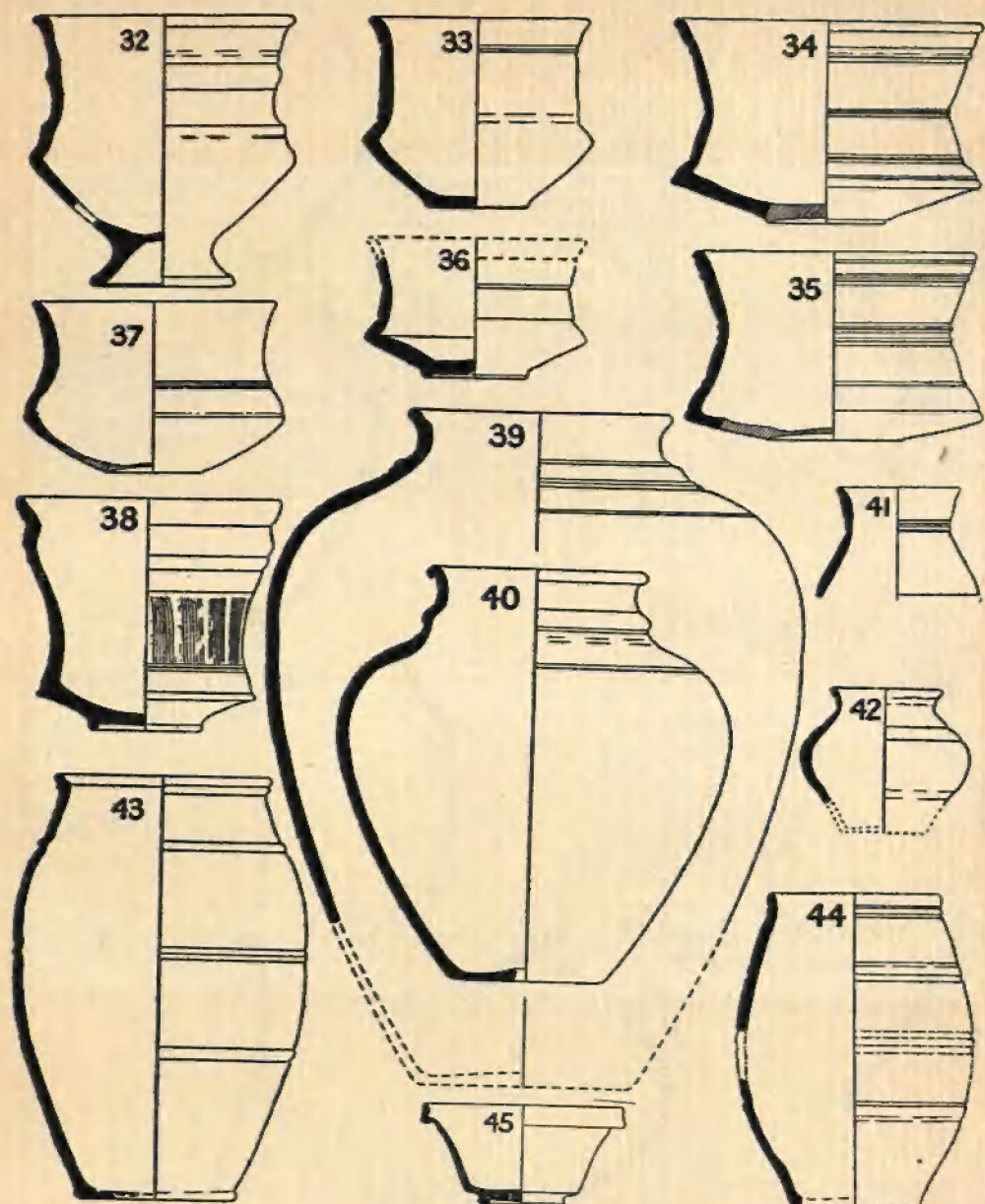


FIG. 7. Pottery: coarse ware $\frac{1}{4}$

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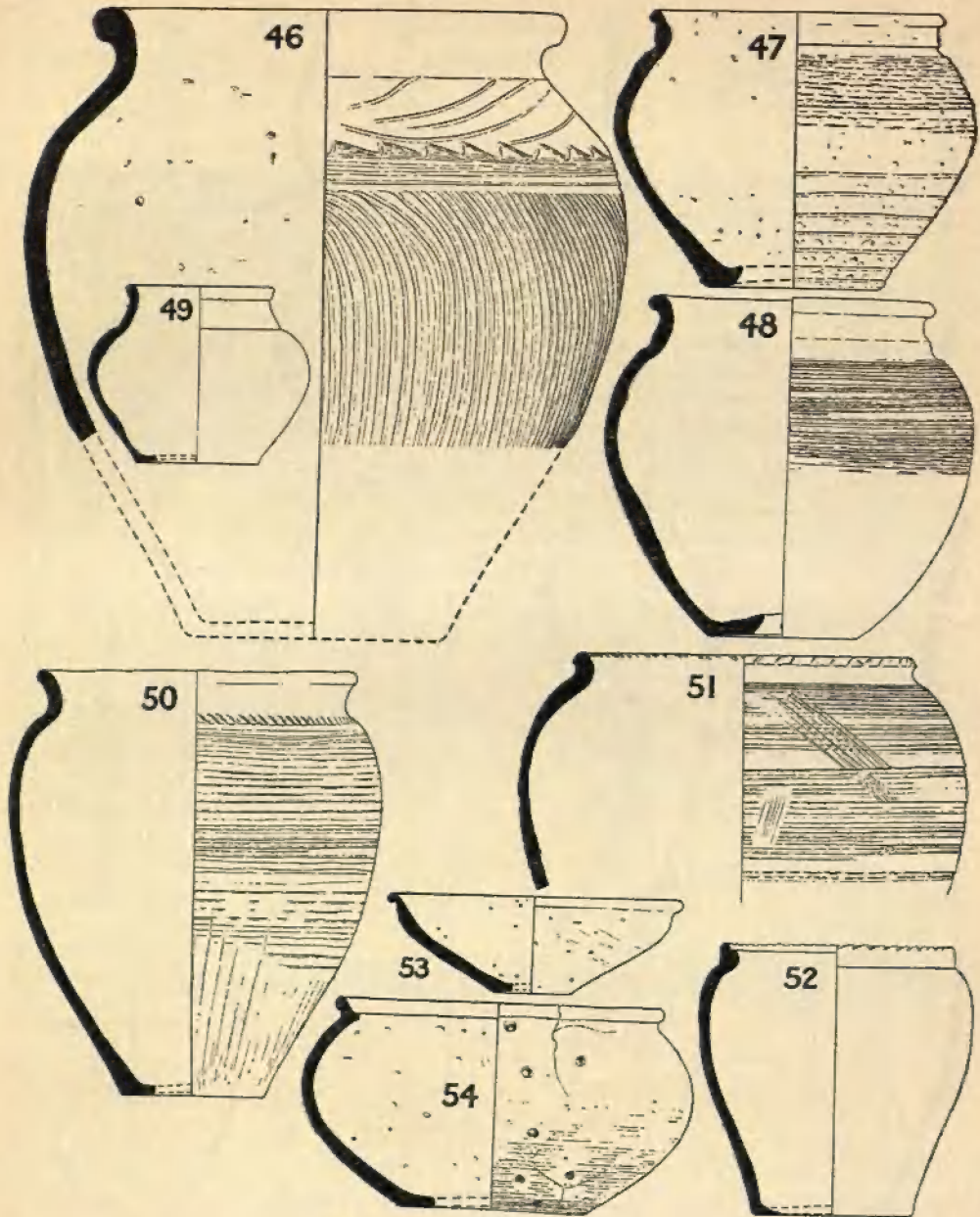


FIG. 8. Pottery: coarse ware $\frac{1}{4}$

A PREHISTORIC METROPOLIS

influences from Italy. This concurrence of Continental traditions at prehistoric Verulamium offers a further illustration of Caesar's statement that the dominant factor in the population of southeastern Britain consisted of settlers from Belgium. It may further be deduced from the character of the associated Belgic and Italic wares that the intensive occupation of the area excavated dates from about 10 B.C. to A.D. 40. It should be emphasized, however, that the area so far explored is relatively small and that, in particular, the areas adjoining the approach to the ford, where we should expect the earliest and most intensive occupation, have not yet been touched. The explorers—to adapt Tacitus—have shown prehistoric Verulamium to the world rather than excavated it. But they have not yet laid down the spade; and it is perhaps relevant to add that the secretary of the Verulamium Excavation Committee (which is appealing urgently for funds) is Mr C. E. Jones, Warners, Russell Avenue, St. Albans, and that its chairman is Sir Charles Peers, President of the Society of Antiquaries.

Balloon Photography and Archaeological Excavation

by P. L. O. GUY

Field Director of the Megiddo Expedition of the Oriental Institute of the University of Chicago

IT is a great help to the understanding of a piece of country, a town or a building to get to some high point in the vicinity and look down upon it, and the more nearly vertical one's point of view the more accurate is one's impression. In the 'nineties, as a boy, I used to spend hours at the top of the spire of Rouen Cathedral gazing down upon the city below, and it was only a step from this to record such a view photographically; but it was some time before I took my first truly vertical photograph.

About 1904, a friend who was engineer on the construction of the Rothesay Dock near Glasgow asked me to photograph for him the concrete foundations of a part of this dock: I could not get a proper view of these from anywhere on the ground, so I asked if I might utilize a great crane which stood nearby. It was probably against all the rules of the Clyde Trust, but I was thereupon swung high in the air over my subject, and plate 1 shows the result obtained with a no. 2 Bullseye Kodak.

During the war air-photographs became a commonplace, and it was my lot to use them a good deal, particularly in connexion with the indirect firing of machine-guns. Later on, in 1922 and 1923, when I was in the Palestine Department of Antiquities, friendly relations with the R.A.F. enabled me to examine a number of ancient sites from the air, discovering such things as the complex of buildings buried by sand-dunes inside the enceinte of the Crusaders' castle of Athlit. The air-photos obtained at that time formed the beginnings of the Department's collection, but they were chiefly what may be termed reconnaissance photos, showing the extent and character of ruins still covered by surface soil.

The excavation of Megiddo presented the opportunity of realizing an old desire—the use of air-photography as an aid to recording ruins

BALLOON PHOTOGRAPHY

progressively exposed, and as a help in the often puzzling task of distinguishing excavated buildings of one stratum from those of another.

The use of aeroplanes or full-size balloons was ruled out for reasons of expense, and I cast about for other means.

Kites offered one solution of the problem, and I have to thank Major B. F. S. Baden-Powell, whose successful work with these during the South African war will be remembered, for giving me information about them. I came to the conclusion, however, that for our particular conditions and requirements a small captive balloon would be more suitable, and by renewing an old acquaintance with the Royal Aeronautical Society I succeeded in getting into touch with Mr Griffith Brewer, of whose early work I had heard. Mr Brewer replied enthusiastically to my enquiries, was good enough to show me several admirable photographs he had obtained, I believe in 1902 or 1903, by means of a camera suspended from a small balloon of gold-beaters' skin, and even offered to rout out his old apparatus for me to look at. My stay in London was too short to permit me to avail myself of this offer but Mr Brewer's verbal explanations were clear, and the apparatus itself simple. The success of the photographs since taken at Megiddo owes much to him.

On my return to Palestine it happened that one of my assistants, R. S. Lamon, was going to America, so I commissioned him to have made an electrical release, controllable from the ground, which would trip the camera-shutter. The apparatus which he procured (it was made by the Physics Department of the University of Chicago) is a beautiful little piece of mechanism: it is light and simple, and it works.

I had also asked Lamon to see if he could not find a small ready-made balloon in America, in order to save the expense of having one built specially, and he bought one of the expandable rubber kind that is used for meteorological purposes. This had the merit of being light and cheap but, as will appear, it turned out to be unsuitable.

Meanwhile O. E. Lind and E. L. De Loach had manufactured a light, fixed-focus camera of three-ply wood to take one of our lenses and a 5 by 7 inch film-holder, and when Lamon returned from America we tried the whole combination, as well as the wooden winding reels made locally to carry the light steel wire (uncovered) which we used both for holding the balloon and for passing the current to the shutter-release.

We inflated the balloon with hydrogen which I had procured in

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cylinders from France : this is an expensive item, not because the gas itself is dear but because of the price of the cylinders and the shipping charges on these.¹ I can see no way of avoiding this, for all the machines for generating hydrogen on the spot, about which I have enquired, are either too large or too small.

On our first attempt we controlled the balloon by wires from two points : it went up excellently and we got two photos, one good² and one poor. Then came disaster. We had got the balloon back into my garage, which we were using as its shelter, and were pegging it down by means of a light fishing net thrown over it. Now whether it was that the balloon, whose skin had certainly become somewhat electrified, generated a spark through friction with the net, or that there was a flaw in the rubber, I know not ; but there was a loudish pop and the balloon, as such, ceased to exist. Perhaps it was merely because it happened to be the fifth of November—an appropriate date for more reasons than one.

We were, however, not at all discouraged, for our experiment had in the main been successful. We had proved that we could take a good photo, and we had learned a good deal one way and another. The main things were that expandable rubber was evidently not the right material for the balloon, that thin steel wire was not good for control because it could kink easily (we later found that it also got quickly rusty), and that our rather primitive winding-reels were not good enough. Our mistake had, in short, been to spoil our ship for a ha'po'rth of tar, and I would warn other experimenters against this.

I hesitated to use gold-beaters' skin for our new balloon because I feared that it would quickly perish in the heat of Palestine, so I decided to have a rather larger balloon made of the heavier but thoroughly solid rubberized silk, and on 25 November 1929 I cabled my requirements to the firm whose name Mr Brewer had given me. They carried out the order with a speed and accuracy that are truly praiseworthy,

¹ While the balloon itself was admitted into Palestine free of duty, under the regulations governing the facilities granted by the Palestine Government to Archaeological Expeditions, I have been officially informed that 'Exemptions accorded to Archaeological Societies are not held to cover the importation of Hydrogen'. The cost is thus increased considerably, for we have been obliged to pay duty at 12% not only on the gas itself but on the cylinders, and also on the freight and insurance of each consignment.

² Published as fig. 10 in Oriental Institute Communication no. 9. (*Chicago : University of Chicago Press, 1931. \$1.*)

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for the finished balloon arrived at Gaza aerodrome on 13 December, having been despatched by Imperial Airways on the 7th. It is true that the Palestine Customs held up delivery for four days, apparently expecting that I would travel 120 miles to Gaza in order to clear it there, but even though I did not do this I received it only twenty-two days after placing the order, and it has turned out to be satisfactory in every way.

Meantime Lamon had designed a better type of winding-drum, with a simple, squeeze-on friction-brake, and we had three made in Haifa. We replaced the original mooring wires by strong cords capable of holding about 80 pounds each, and also prepared a third attachment of insulated copper wire (double). This last was intended solely to carry the current for the shutter-release and to act as a height-gauge, but not to take much strain. Since the new balloon had more lift than the old one, and could carry about 12 pounds, Lind made a new camera (also of three-ply wood) to take an 8 by 10 inch film-holder.

Our whole apparatus, with its cost, is as follows :—

Balloon ³ of rubberized silk, 12 feet in diameter, 900 cubic feet capacity, made by the R.F.D. Company, 17 Stoke-road, Guildford, Surrey	£50
Camera of 3-ply wood completed with oddments, estimated cost if purchased (without lens)	£15
Electric shutter-release	\$50
Battery for operating this, composed of ordinary torch batteries, with switch and connexions ⁴	about £1
150 metres insulated copper wire (double)	} about £2
300 metres stout cord (not just string)	
3 winding reels	£5
6 Hydrogen cylinders, holding 7 to 8 cubic metres each ..	francs 2700
Manometer for checking contents of cylinders and controlling rate of inflation of balloon	francs 175
Hooks and oddments	under £1
Shed of wood framing and asbestos sheeting ⁵	about £70

We use a pretty fast film (Eastman Portrait super-speed) in order to get as short an exposure as possible; this avoids muzziness if the camera happens to move a little at the critical moment.

³ This balloon is spherical. Mr Brewer has since suggested that a 'sausage' would work just as well, and would require a much smaller shed. I agree.

⁴ Lamon is trying to get hold of a magneto to replace this: it would never run down, and would enable us to release the shutter merely by turning a handle. About 15 volts would suffice.

⁵ This *must* be sparrow-proof and mouse-proof.

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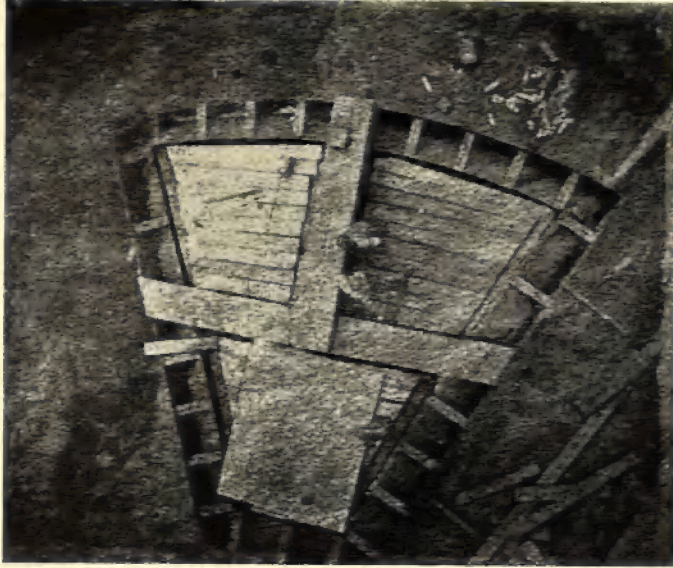
The balloon as supplied had a wide filling-tube below, and this had an open mouth which could be loosely closed by a cord. We found that, in going up and down, gas was lost, or air admitted, or both, and that the lift decreased, so we sealed it up tight with tire-repairing material, and fitted a motor-tire valve (without the inside) for inflating, and for letting out a little gas on expansion. This valve is kept closed by the ordinary type of cap.

The rigging is simple. There are 3 equidistant leading lines for walking the balloon about, and 4 lines which originally led down to a wooden hoop about 30 cms. in diameter. We found it better to replace this by a stout metal snap-hook. The camera is suspended by two spliced cords and one double electric wire, all about 50 cms. long, from a metal ring some 5 cms. in diameter : this three-point suspension facilitates adjustment for level—it is desirable that the film-holder should be absolutely horizontal. The ring is attached to the balloon snap-hook, and to the ring are attached snap-hooks at the ends of the two mooring lines. The electric wire is also attached to the ring by a snap-hook, but is insulated from it : this arrangement relieves the electric connexions with the camera of all weight and strain. For the connexions small 'wireless' plugs are used, different colours distinguishing positive from negative. (Plate II).

We go to work in this manner. Choosing a still morning—the air *must* be still—we inflate the balloon slowly through the manometer to about the degree shown in plate III which allows for normal expansion, and we close the valve. All the apparatus is then taken to the middle of the area to be photographed ; the camera, mooring lines and electric wire are all attached ; the slide is drawn and the slit for it is blocked by a piece of cardboard cut to fit exactly so as to exclude light ; the shutter and shutter release are set, a scrap of paper is jammed behind the shutter-trigger and the plugs for the connexions are inserted. When each of these details has been checked the balloon is allowed to rise slowly and without jerks, special care being taken to prevent these at the start.

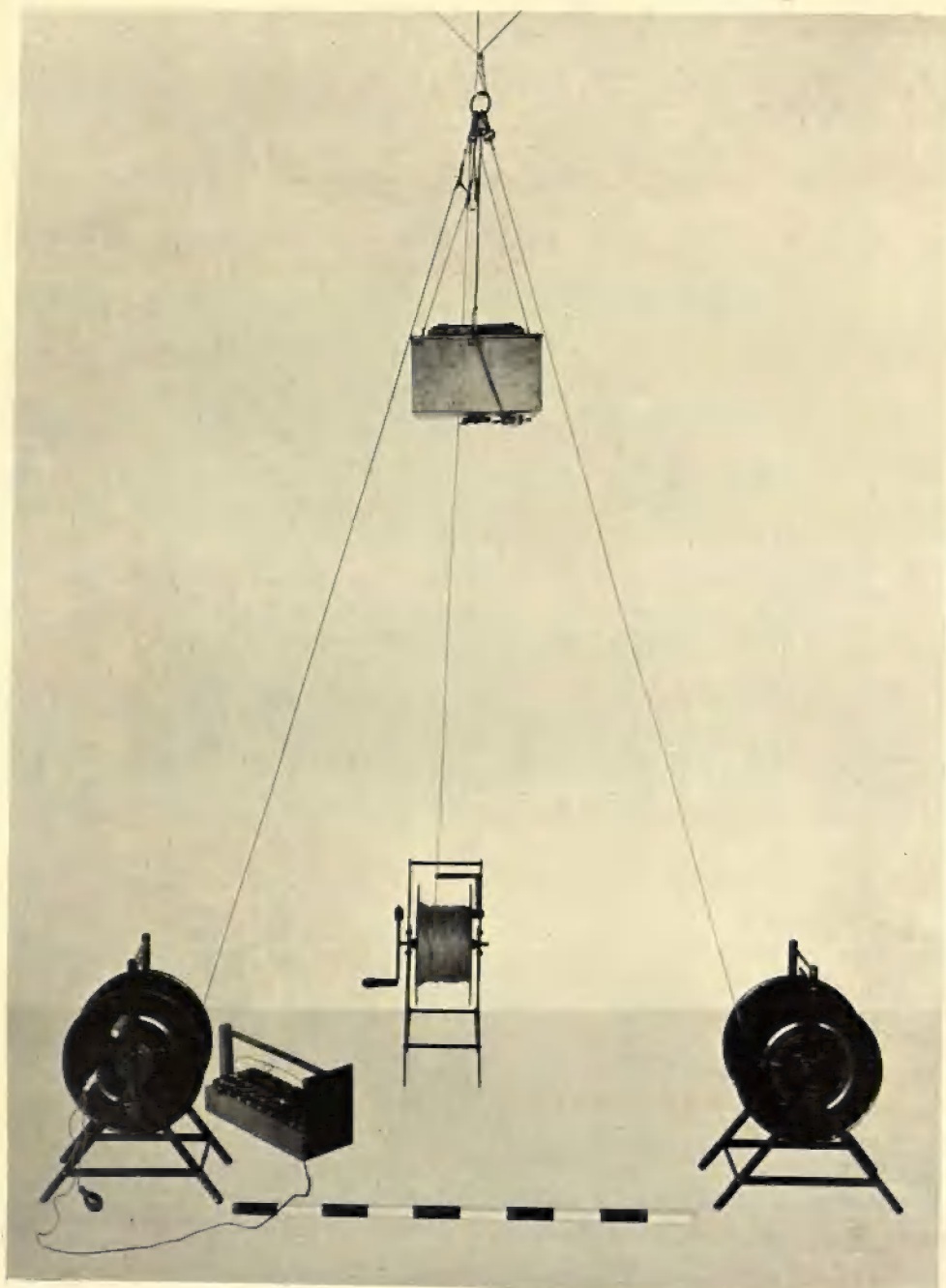
The battery-man and I remain at the middle of the area to be photographed, and the men at the mooring lines carry their reels outwards, and let out or take in line according to a simple code of hand-signals given by me. They should wear hedgers' gloves, or at least have a piece of sacking, for handling the line. The battery-man manoeuvres his reel so as to relieve the electric wire of strain. This wire is marked at 50-metre intervals, and when 100 metres are out,

PLATE I



OVERHEAD PHOTOGRAPH OF FOUNDATIONS, ROTHESAY DOCK

PLATE II



CAMERA AND WINDING REELS

The electric shutter-release can be seen to the right of the lens. The left-hand reel carries the electric wire, and the battery box is beside it. The friction-brake can best be seen at the top of the centre reel; by squeezing the bell-crank against the carrying-bar a block of fibre is pressed against the right-hand side of the drum. Scale, 1 metre long.

Copyright by the Oriental Institute of the University of Chicago

PLATE III



BALLOON READY FOR USE, WITH CAMERA ATTACHED. THE OLD TYPE OF REEL IS SHOWN.
Copyright by the Oriental Institute of the University of Chicago

PLATE IV



EXTENSIBLE LADDER USED AT MEGIDDO FOR OBSERVATION AND PHOTOGRAPHY
Height 9.7 metres

Copyright by the Oriental Institute of the University of Chicago

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and the balloon is over my head, I tell the battery-man to make contact. If, on doing this, the scrap of paper does not fall from behind the shutter-trigger, he moves his wires along so as to take current from more cells in his battery until the paper does fall. This, of course, indicates that the exposure has been made, and we then wind in, change the film and pass on to the next area to be photographed.

It is better to work as quickly as may be without hurrying, and to get as many photos as possible in the same morning, for the weather may change and the balloon does lose lift in time. Up to a point it may be topped up by the admission of more hydrogen, but after a while the effects of osmosis become pronounced and then it must be completely deflated and refilled. This indicates that if one has very much photography to do one must have a reserve of full gas cylinders. Cylinders, by the way, need checking by manometer to see that they are really full—we once received a consignment which had evidently been tampered with *en route*, and it took the contents of all six to fill the balloon to the degree normally reached by three. As a general rule we find that in a single morning we can take as many photos as the progress of excavation requires.

In order to be able to make exposures in quick succession the ground must naturally be prepared beforehand—any weeds that have grown up in odd corners must be removed, and the whole area must be properly cleaned up and examined before the balloon is inflated. The area one's particular lens will cover at the height it is decided to use must be known, and the points to be the centres of the different photos, at which one will take one's stand for control purposes, must be fixed in advance.

It would be better to have a square film than an oblong, for one cannot control the exact position of the camera, which sometimes develops a slow spin, and in making calculations for area covered one can only take into account the shorter side of the film. For this purpose I usually draw circles on a squared plan of the dig, and take with me a slip on which I have noted the position of the centres of these.

The site of Megiddo is divided for survey purposes into 25-metre squares: these squares are drawn on our plans, and their corners are marked on the ground by pegs. I had placed a hollow concrete brick round each peg, for protection, and we have found that these bricks show up very clearly and distinctively on the photos. They are very helpful in preparing our finished prints.

It is more difficult than one would suppose to get the balloon at

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exactly the same height for all photos, and occasionally the exposure is made at a moment when the camera has swung a little out of the horizontal, making the squares smaller at one end than at the other. In practice, however, neither of these things matters in the least, for we do not use contact prints much, but enlarge to a scale of 1 : 250, and any necessary corrections for scale and distortion are made by focussing the enlarging camera, and tilting the enlarging board, until the hollow bricks on the image coincide with the corners of squares drawn to this scale upon the board.

Apart from some 17,000 square metres which have been dug for dumping on, on the slopes round Megiddo, the whole summit of the mound, which has about the same extent (13 acres) as the Tower of London including the moat, is under excavation. We have photos, enlarged to the 1 : 250 scale, showing this area as it looked last spring, and Lind has made up two mosaics from these measuring 1.3 metres across. One has been sent to Chicago as a record; the other is a working copy, and as such it is in constant use on the dig and in the drawing office.

Megiddo is a deeply stratified site dating from about 350 B.C. to chalcolithic times, and we are excavating it, stratum by stratum, over its whole area. Most of the walls we are finding at present are of rubble and mud, and such a wall belonging to one period can look abominably like one belonging to another, especially if it happens to be connected with nothing in particular. It will be understood that a large-scale air-mosaic is a very great help in disentangling one stratum from another: for one thing it often makes it possible to see which walls belong to a particular system of alignment, and for another, it permits one to compare buildings which are perhaps a hundred or two yards apart—having spotted a similarity on the mosaic, one can walk over and verify it (or not) on the ground.

We of course do what we can to date buildings by the pottery and objects found among them, but in addition, and for making the plans, we proceed thus. Lamon (who does most of the surveying) and I go on to the site taking the mosaic with us, and compare it with the ruins on the ground. We decide which remains belong to say stratum 1, and mark them with a splash of paint, making a corresponding ink mark on the mosaic. The marked ruins are then planned and levelled in the ordinary way, and here also the mosaic is helpful for detail. When the plan is complete the marked ruins are removed; what lies below is re-photographed, and the whole process repeated.

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There is another gadget which I have found useful both for photography and for observation: it is an extensible ladder, of the type used for cleaning street lamps, which winds up to a height of 9.7 metres, and it gives a good view over a considerable area. I have had its stability increased by fitting four really stout guy-ropes, and have proved it to be safe even in a fairly strong breeze. (Plate IV).

Running through these notes I see that I must apologize for their rather scrappy nature: they lack the finish that one has come to expect in articles accepted by the Editor. But excavators who may happen to read them will perhaps excuse this when I explain that they have been jotted down at odd moments while the dig is going on.

There is one thing that worries me. We have got quantities of fine, large-scale air-photographs of Megiddo, and we shall have lots more, and shall make a mosaic of each stratum. The originals can be seen and studied by people who happen to be in Chicago, where one set will be kept, or who visit us here on the site. But I can't quite see how I am going to publish them, at their useful scale of 1:250, without producing a book or portfolio that only a son of Anak could handle.

At present my idea is to publish a selection of them on a reduced scale in a volume of normal archaeological format and to provide, as a kind of supplement, to be purchased optionally at a small extra cost, a set of lantern slides which those who wish can throw on a screen. Slides are all too often used only once, at a lecture, and are then put away in a box. Few people bother to get out a lantern and use the projected image for quiet study at close range: it is a method I have tried and can recommend.

But whatever means we may find for publishing our balloon-photos (we shall find one somehow) the main point about them is this—and all diggers who have seen them agree about it—they have a high and lasting archaeological value.

The Kelts in Britain

by IORWERTH C. PEATE

Assistant Keeper, National Museum of Wales

SIR JOHN RHÛS, whose name is justly honoured by all students of the history of Britain, is nevertheless indirectly responsible for much vague conjecture concerning the Keltic problem by English archaeologists. I write 'English' advisedly, for Irish archaeologists—with a profound knowledge of Keltic philology—have definitely refuted the theories propounded by RhÛs and maintained in various modified forms by subsequent archaeologists in England. In Wales, the other Keltic country concerned, archaeology unfortunately remained up to recent times the happy hunting-ground of antiquaries whose knowledge of both archaeology and philology was very restricted. Her professional archaeologists, in more recent years, have been hampered by an ignorance of the philological problem and have therefore naturally subscribed to the point of view of the modern English school of thought.¹ On the other hand, some Welsh philologists, often with little or no knowledge of the archaeological evidence, have ridiculed the theories advanced by RhÛs and the later English writers on the subject. My present purpose is to submit to the readers of *ANTIQUITY* a case for relinquishing entirely the generally accepted attitude of modern English archaeology towards the Keltic problem.

RhÛs,² elaborating a theory advanced by Edward Lhuyd³ in the 17th century, postulated that the island of Britain was invaded by two waves of Kelts. First came the Goidels who settled here; then followed the Brythons. Finding that the consonant *q*, where it occurred in the ancient forms of the Irish language, was replaced by *p* in the corresponding forms of the ancient British language, RhÛs

¹ But it should be noted that in 1925 Dr R. E. M. Wheeler, then Director of the National Museum of Wales, wisely refused to commit himself on the subject (*Prehistoric and Roman Wales*, pp. 6-7). Professor V. Gordon Childe's admirable summary (in *The Bronze Age*, pp. 242 *et seq.*) should also be noted.

² See, for instance, *The Welsh People*, chapter 1.

³ In his Welsh preface *At Y Kymry*.

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divided the Kelts into two linguistic groups which he named the Q-Kelts and the P-Kelts. Now this linguistic classification, which is acknowledged as fundamental by all philologists,⁴ was projected by Rhys into the archaeological field, and for his purpose he utilized the term 'Goidel' to represent the Q-Kelts and 'Brython' the P-Kelts, thus giving to a fundamental philological occurrence an archaeological and geographical significance for which there was, as will be seen later, no justification. Rhys's chronology of course need not concern us here for it has been abandoned, as far as I am aware, by all archaeologists.

But this dangerously unsound archaeological torsion of a simple philological classification has been tacitly accepted in many quarters. Déchelette⁵ refers to 'la première invasion de la Bretagne insulaire admise par les celtistes, celle des Goidels'. O. G. S. Crawford⁶ suggests that invaders arriving about 800-700 B.C. 'may have been Goidels'.⁷ H. J. E. Peake⁸ accepts the P and Q theory almost in its entirety. He states that archaeological evidence proves that 'the thesis of Sir John Rhys that two waves of people left Central Europe for Italy and the west, the first speaking a Q and the second a P tongue is absolutely correct'. The authors of *St. Catharine's Hill*, though expressing a desire 'to avoid the tangled linguistic controversy (!) involved' also accept the classification.⁹ Indeed without referring to any other authority, it may be said that the P and Q classification has become an accepted formula in modern archaeology.¹⁰

⁴ The *p* and *q* classification in philology is well established. Labio-velar *qu* becomes *p*, e.g., Gaulish *penno-*, Welsh *penn* 'head' but Irish *cenn*; Gaulish *pempe-*, Welsh *pump* 'five' but Irish *cóic* (cf. Latin *quinque*); Gaulish *-epo-*, Welsh *ebol* 'colt' but Irish *ech* 'horse' (cf. Latin *equus*). See, for instance, Georges Dottin, *La langue gauloise*, p. 98.

⁵ Vol. II (Hallstatt), p. 76 and note.

⁶ *Antiq. Journ.*, II, 27.

⁷ Later on however (*Antiq. Journ.* II, 207) Crawford, in the light of new evidence from Ireland, is 'prepared . . . to look for another name to distinguish (the) invasion'.

⁸ *The Bronze Age and the Celtic World*, chapter 8.

⁹ pp. 151, 161.

¹⁰ A prominent anthropologist, Professor F. G. Parsons, D.Sc., in a lecture for the Royal Anthropological Institute in 1930 (see *Early Man*, p. 72) provides an outstanding example of the uncritical way in which the theory has been accepted. Parsons writes: 'Two waves of Celts are recognized. Firstly, the Goidels, Gaels, or "Q-Celts", (who) quickly passed or were driven from England into Ireland, the Highlands of Scotland and the Isle of Man, where their language, abounding in *k* and *q* sounds, (!) is still heard. Secondly, the Brythons, Britons, and "P-Celts" who used a *p* or *b* where the Goidels used a *k*. These people, who called themselves Cymry, spoke a language akin (!) to modern Welsh'.

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But this classification before (approximately) the 3rd century B.C. is fundamentally unsound and no modern student of Keltic philology can possibly accept it. Professor O. J. Bergin's statement¹¹ that 'there is not enough Old Gaulish material extant to solve the problem of the early distribution of the Q- and P-Celts, or the date of the change from Q to P' is a very temperate statement of one aspect of the case. Phonetic changes such as the substitution of P for Q spread, as Eoin MacNeill¹² suggests, 'in an almost mysterious way through languages. Their spread may be arrested by a geographical barrier so considerable as the Irish Sea, but it [is] not at all likely to have been brought to a stand by the waters of the Seine and Marne'. But a belief in such an arbitrary arrest is necessary to accept Rhŷs's archaeological interpretation of a perfectly justifiable philological classification.

It can be stated definitely that no case for a clear-cut distinction of the nature posited by Rhŷs to have existed between the language of the western Kelts and that of the Belgic Kelts on the continent can be defended, and I venture to believe that, with the added light of recent philological and archaeological research, Rhŷs would himself have abandoned his theory as he abandoned many other theories. 'He himself never regarded a theory as more than a working hypothesis; and no one was ever readier to discard a theory when it was found not to be consistent with all the facts'.¹³

All workers in archaeology welcome the results of the researches of present-day archaeologists into the incidence of the prehistoric invasions of Britain. Of the invasions there can be no doubt; but the modern archaeologist is open to just criticism when he equates archaeological periods with philological events for the dating of which there is no evidence.

Assuming the invasion dated by Crawford¹⁴ at about 800-700 B.C., by other writers two or three centuries earlier and by others later by a similar period, to be Goidelic, there is no reason, as Professor Eilert Ekwall¹⁵ points out, why there might not be some Goidelic place-names in England preserved from the time of the early settlements. But although there is evidence of Brythonic place-names in England, there is, as far as I know, no evidence of Goidelic names. Ekwall states

¹¹ Quoted in *Antiq. Journ.* II, p. 205.

¹² *Phases of Irish History*, p. 46.

¹³ Sir J. Morris-Jones, *Sir John Rhŷs Memorial Lecture*, p. 12.

¹⁴ *op. cit.*, p. 27.

¹⁵ *Introduction to the Survey of English Place-Names*, p. 32.

THE KELTS IN BRITAIN

that 'no certain Goidelic names have so far been pointed out', and Dr O. K. Schram¹⁶ maintains that he is 'certain that nothing further will come to light that will falsify Ekwall's definite pronouncement'. It may of course be held that the Goidelic language and place-names must have been totally superseded by Brythonic, but such a negative argument would have little validity without a strong backing of positive evidence which is, of course, not forthcoming. Rhÿs, however, overcame this difficulty and fortified his argument by attributing the Ogam inscriptions to the descendants of the Goidels who had held their ground in Britain. But this untenable theory has long been discarded. Indeed the only 'Goidelic' elements found in the English place-nomenclature are due to the much later influence of the Viking settlements, in the northwest and north especially, and of Irish missionary enterprise along the west coast.

Some archaeologists have gone further, and, following Rhÿs, have maintained that the Celtic settlement of Ireland was effected through Britain, though E. C. R. Armstrong¹⁷ has shown, with much cogent argument, that 'the view that Ireland was not colonized by the Celts until the late Celtic period demands consideration', and has emphasized the complete absence from Ireland of the later Hallstatt material. Dr R. A. S. Macalister,¹⁸ corroborating this view, adds in the true scientific spirit: 'Nothing can be assumed against the possibility of such a discovery [as All Cannings Cross] in Ireland: although it may fairly be remarked that it would prove subversive of all the theories of occupation that have been forming themselves on the basis of the knowledge hitherto acquired, and that seem to fit the facts so far as they are known'. He maintains that 'the absence of Hallstatt pottery strongly weighs the balance against the presence of Hallstatt people'.¹⁹

There is, of course, no reason to suppose that there was no relation

¹⁶ In a letter to the writer, December 1931.

¹⁷ *Antiq. Journ.*, II, 205-6.

¹⁸ *The Archaeology of Ireland*, p. 133.

¹⁹ In a recently published work (*Tara: a Pagan Sanctuary of Ancient Ireland*, p. 90), Macalister writes: 'Archaeological evidence dates (the Celtic occupation of Ireland) about 400-350 B.C., but the old historians would have us believe that it took place about 1300 years earlier'. A view similar to that of the 'old historians' was expressed in 1921 by M. J. Loth (*Revue Celtique*, 1921, p. 288) who referred to the first Celtic invasion of the isle of Britain 'au commencement du deuxième millénaire avant notre ère'. It may be remarked for the guidance of those archaeologists who regard this pronouncement seriously that it is entirely discounted by the great majority of Celtic philologists.

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between Ireland and Britain in prehistoric times or to conjure up an insuperable barrier between the two islands as is implied in the writings of Zimmer,²⁰ Meyer²¹ and their school. The presence in Central Wales of such artifacts as arrowheads of Antrim flint,²² of Irish type and of Bronze Age date, alone proves otherwise. But without participating in the excesses of some of the writers of the Meyer school it may be held that the origin of the *via*-Britain theory of the Celtic colonization of Ireland seems to have been founded on a belief that the only way to Ireland from the continent is through Britain²³—a view inconsistent with the evidence of prehistoric migrations. This aspect of the Celtic problem has however to be judged in the light of philological as well as of archaeological evidence, and one cannot fail to accept Ekwall's pronouncement²⁴: 'The theory [that the Goidels were partly driven westward to Ireland from Britain] is not founded on very strong arguments. The theory . . . that they actually did reach Ireland direct from France or Spain, not by way of England, seems to the present writer to have considerable probability'. It may be added that the Old Irish numerals, still in use by shepherds in Cardiganshire, which have been examined in detail by my friend Professor Alf Sommerfelt²⁵ of the University of Oslo, present no evidence of Goidelic survivals in Wales which cannot be accounted for by invasions from Ireland.

The conclusions which I wish, therefore, to draw are: that modern archaeologists should abandon all attempts to equate invasions for which there is archaeological evidence with philological events for the dating of which there is no evidence; that an arbitrary classification of the Celtic peoples of prehistoric times into Q-Kelts and P-Kelts is—in an archaeological context—fundamentally unsound, and that archaeologists who—in their ignorance of Celtic philology—wittingly or unwittingly maintain this method of nomenclature are open to ridicule; and, finally, that the whole problem of the Celts in Britain, insoluble without the help of philology, should be re-examined by archaeologists in the light of these conclusions.

²⁰ *Auf welchem Wege kamen die Goidelen vom Kontinent nach Irland?*

²¹ *Trans. Hon. Soc. Cymmrodorion*, 1895–6.

²² *Arch. Camb.*, 1930, p. 200.

²³ *cf.* for example T. Rice Holmes (*Ancient Britain*, p. 446): 'It is hard to believe that they would not have directed their immigration towards Britain, the nearer country'. See also G. Coffey, 'Archaeological evidence for the intercourse of Gaul with Ireland before the first century' in *Proc. Roy. Irish Acad. Sect. C.*, xxviii, 96–106, and Zimmer in *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften*, 1909.

²⁴ *op. cit.* p. 32.

²⁵ *Des noms de nombre irlandais au Pays de Galles*. Oslo, 1925.

British Hanging-Bowls

by T. D. KENDRICK

THE first person to introduce these bowls to the archaeological world as a group, and with appropriate comment, was that profound student of Celtic art, the late Mr J. Romilly Allen¹; subsequently, Mr Reginald Smith set himself the task of making a complete list of all the bowls, or parts of bowls, that were known², and he has since placed us further in his debt by the care he has taken in seeing that every new example, as it came to light, was adequately recorded. Since Mr Allen wrote his paper, however, we have not made very much headway with the main problem of deciding their position in the cultural history of this country; even the recent discovery of the beautiful Winchester bowl, that excited us so much in 1930, was allowed to pass without any revision and revaluation of the really astonishing material that had been collected by Mr Smith, and it seemed that the chief interest aroused by the bowls was for ever to be focussed on the apparently insoluble problem of their use. But last year my friend Mr J. D. Cowen published a paper³ that constitutes in many respects a very important advance; it was the stimulating kind of article that often provokes other people into rival activities, and I hope it will not be counted against him if I admit that one of his results was an immediate decision on my part to put into print the notes I had collected on this same subject.

It is Mr Cowen's distinction that he is the first to isolate and appreciate the 'heater' group of bowls, to which I shall refer below, and there are several other points in his very vigorous paper that strike me as being valuable and sagacious observations. But what I have to say here and now is that if Mr Cowen is right throughout, much that I am going to say below is completely wrong. I must explain, therefore, that it is fascinatingly easy to be wrong about

¹ *Archaeologia*, 1898, LVI, 39.

² *Procs. Soc. Ant.*, 1907-9, XXII, 66.

³ *Archaeologia Aeliana*, 1931, series 4, VIII, 329.

hanging-bowls, because some very hard work has got to be done before we begin to understand them properly ; so, having uttered this caution, I will say further that I do not think I shall be very apologetic if I am eventually numbered among those who have erred. The really important thing, as Mr Cowen will agree, is to formulate at least some sort of theory that attempts honestly to answer the fundamental question as to date and origin ; we must, we feel, rid ourselves of the charge of being frightened by these bowls. That is why I have purposely stated my views with imprudent outspokenness, knowing that all bowl-lovers will sympathize with my difficulties, and will perhaps forgive me for the omission of numerous reservations and qualifications that caution would have had me insert.

Hanging-bowls of bronze, already known in this country during La Tène times,⁴ returned into fashion here in the second half of the Roman period. There was a simple and nearly hemispherical type of bowl of beaten bronze, with a cupped base and an in-turned rim, that was popular at this time in Roman Britain (to the best of my knowledge it is not found abroad), and it could be made to hang by fastening hooks and chains to it. You can see examples, without the hanging-attachments, in a hoard of bronze vessels (now in Northampton Museum) from the Romano-British settlement-site at Irchester,⁵ and I illustrate one of them here, together with a strainer from the same hoard (pl. I, 1 and 2). An approximate date, about A.D. 400, is suggested for its hiding by the inclusion in the group of a standing bowl with a flanged and embossed rim⁶ ; but you can also compare the strainer with another in the Kingadle hoard⁷ (found with 3rd century coins) from Wales, or look up the Halker group⁸ of bronze vessels from Flintshire (which includes a bowl of Irchester type and a handled bowl with triangular ears, also in use about 400) in order to satisfy yourself

⁴ A handsomely engraved 4-loop bowl, with a flange and collar made separately from the body and joined to it by rivetting, comes from Denbighshire ; it was made in the 2nd cent. B.C. *Antiquaries Journal*, 1926, VI, 276.

⁵ *Associated Architectural Societies Reports*, 1875, XIII, 89.

⁶ For this bowl-form abroad see *Germania*, 1931, XV, 259, and Lindenschmit, *Handbuch*, 1889, I, 479.

⁷ *Archaeologia Cambrensis*, 1901, series 6, I, 24. The Kingadle strainer is, however, of an earlier type than that from Irchester ; cf. H. Willers, *Neue Untersuchungen über die Römische Bronzeindustrie*. Hannover, 1907, p. 84.

⁸ *Archaeologia*, 1803, XIV, 275, pl. XLIX.

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further that the Irchester bowl-form is likely to be late Roman. There are, of course, several examples from other places; one (Maidstone Museum) comes from an Anglo-Saxon grave at Lower Shorne in Kent; one comes from the hoard found at Wotton⁹ in Surrey, and another from the Sturmere find¹⁰ in Essex, and in both these instances the hoards include frying-pans with folding handles that are late Roman (or even Coptic), rather than very early Roman.¹¹

The Irchester type of beaten bronze bowl can be seen complete with attachments for hanging; two examples are among a group of

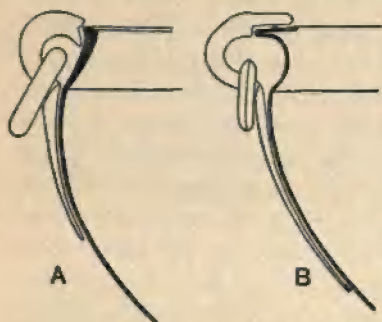


FIG. 1. A. CHESSEL DOWN
B. HAWNBY

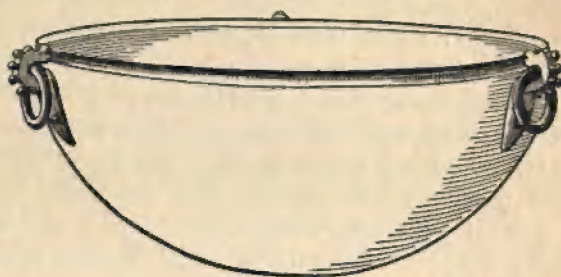


FIG. 2.
FINNINGLEY (D. 12½")

vessels found at Finningley in Yorkshire, one comes from an Anglian grave at Sleaford in Lincolnshire, one from the Jutish cemetery on Chessel Down in the Isle of Wight, one from Faversham in Kent (probably from a Jutish grave also), and another (pl. I, 4) comes from the Grind Low barrow in Derbyshire. There is also an imitation bowl of this form, made of cast, not beaten, bronze, that comes from Barlaston in Staffordshire. In the Grind Low bowl the hook-attachments, which had circular escutcheons, are now missing, but you can still see the scars that they have left; in the Faversham and Barlaston bowls the escutcheons are also round and are enamelled; in all the other

⁹ *Procs. Soc. Ant.*, 1914-15, XXVII, 76. The usual dating for this hoard is Early Iron Age, and the large vessels therein are compared with the big vessel that contained the late La Tène Santon Downham hoard; but this, like all authentic La Tène vessels of respectable size, belongs to the composite sheet class, whereas the Wotton vessels were beaten out in one piece.

¹⁰ *Archaeologia*, 1809, XVI, 364.

¹¹ Cf. *American Journal of Archaeology*, 1921, XXV, 44.

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instances they are heater-shaped and not enamelled.¹² On examining the bowls of this series you notice that the rim-form is not really suitable for the attachment of the fittings ; the end of the hook does not fit very comfortably against the lip of the vessel, for the receding rim does not allow the notch or 'bite' in the terminal to get a firm hold of the edge. Most hanging-bowls, accordingly, differ from the original Irchester pattern by having a wide and flattened lip, this flat top being a fold of the metal sheet pressed inwards and downwards into a horizontal position ; the terminal of the hook could then take the form of a long flat-bottomed snout that would rest firmly on the broad surface of the lip. A comparison between the Chessel Down bowl and a hanging-bowl from Hawnby in Yorkshire will illustrate this alteration in rim-form (fig. 1). Apart from this change, however, the bowl itself remains very much the same ; the body becomes a little fuller, so that the profile tends to become rectangular rather than triangular ; the cupped base becomes deeper and has a flat roof ; but even if you look at the grandest and latest bowl in the whole series, you will see that you are still dealing with a near relative of the simple Irchester vessel.

There is no doubt that loop- and hook-fittings on the edges of metal bowls and vessels, either for the attachment of suspending chains or (if in close-set pairs) swing-handles, were not uncommon in the Roman provinces during the Late Empire. The more elaborate fittings take two forms ; one has an elongated pear-shaped escutcheon and a hook in the form of a bird's head bent *outwards*, and the other short escutcheons of varied shapes with a hook in the form of a lion's head or a snouted creature, bent *inwards*. In our island you can see examples of the first in the Traprain Law hoard,¹³ and also, so Mr A. O. Curle tells us, among the Roman antiquities from Cirencester ; there are specimens of the second kind, mounted on bowl-rims, in the celebrated late Roman finds made at Sackrau¹⁴, and there is a very good example, showing a fine lion's head and a triangular escutcheon, from this country in the Silchester collection at Reading. We have also a considerable number of elongated triangular plate-attachments bearing

¹² Strictly speaking, I suppose, heater-shaped should mean triangular ; but for convenience I am following Mr Cowen and making the term include also the elongated pear-shaped type of escutcheon.

¹³ A. O. Curle, *The Treasure of Traprain*. Glasgow, 1923, p. 38, pl. xvii.

¹⁴ Grempler, *Der Fund von Sackrau*. Breslau, 1888.

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simple round loops, which may have served the same purpose as the more ornamental fittings. There is nothing very remarkable, then, in the fact that hanging-attachments should be added to native Romano-British bronze vessels of the type of the Irchester bowl, and it is not by any means absurd to suppose that our earliest hanging-bowls were made before the Roman period was over.

One of these early bowls comes from Sleaford in Lincolnshire (pl. 1, 5); it is of the type that still has the simple in-turned rim, and it had four (two are now missing) miserable little hook-attachments that are obviously an imitation of the fastenings with out-turned bird's heads such as we find in the Esquiline treasure, or on the Traprain Law dish (pl. 1, 3), which has four hooks like the Sleaford bowl. This vessel was found in an Anglian grave, but it was old and patched before it was buried, having, in fact, the worn-out bottom replaced by a new piece of metal. There is no reason why it could not have been made in late Roman times, though there is no proof that it was. We are interested in it chiefly because it is the solitary example of a native bowl that is fitted with out-turned hooks. All the other bowls that we are going to notice have the hooks turned inwards, like the Silchester fitting, though the heater-shaped escutcheon and the bird's head of the Traprain type of fitting were adopted for regular use by our bowl-makers.

The two hanging-bowls from Finningley are, I think, the earliest that we have in this country; my reason for saying this is that they were found in a group of vessels which included three examples of the standing bowls with flanged and embossed rims, such as we have already noted in the Irchester hoard. The two bowls are very much alike, and I illustrate only one of them here (fig. 2¹⁵). It is of the primitive Irchester form and it has three escutcheons (the Sackrau, not the Traprain number) with lively little animal-heads as hooks, these being provided with 'manes' in the tradition of the Celtic 'fire-dogs'. One of the bowls is a 'water-clock' as the prick of the turn-table or pivot on which it was beaten has perforated the cupped base,¹⁶ and the other has a dent that very nearly, but not quite, turns it into a 'water-clock'. It seems to me to be not improbable that the date of these two bowls may be as early as the 4th century.

¹⁵ From a photograph kindly supplied by Dr W. Collinge of the Yorkshire Museum.

¹⁶ Cf. A. Götze, *Die altthüringischen Funde von Weimar*. Berlin, 1912, p. 19, fig. 15.

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Another instance of the Irchester type of bowl adapted for hanging-purposes comes, as I say, from Chessel Down in the Isle of Wight (pl. II, 1); this has faceted sub-triangular escutcheons with animal-headed hooks (fig. 1, A) which I take to be a version of the Sackrau type of fitting, such as was found at Silchester. It also has a perforated base,¹⁷ like the first Finningley bowl, and there is every reason to suppose that it was made at an equally early period. To illustrate the hanging-bowls with heater-shaped escutcheons that were made at a later date and possess the folded-over rim, we have the Hawnby vessel from Yorkshire (fig. 1, B), and the magnificent bowl in the Oslo Museum from Skomrok in Lyngdal, which has a diameter of no less than 18½ inches (pl. II, 2); this was found in a Viking grave with, among other things, a glass linen-smoother, and may have been a very old bowl when its last owner died. Like several other bowls of this type found in Norway, it had been strengthened by an iron band round the rim. We must also note that, like two bowls found in this country (Capheaton and Basingstoke), its base bears an engraved rosette composed of intersecting semicircles.

So many foreign objects were imported into Norway at the end of the Roman Iron Age, including late Roman types of bronze bowls, that I do not think we need regard all the hanging-bowls of the Skomrok type as being necessarily loot obtained by the Vikings; some of them may quite easily have reached Norway long before the Viking Period began, and I am inclined to think that we ought to date the Hawnby and Skomrok bowls to about the 6th century, and not much later.¹⁸ It may be taken for granted, however, that the fold-over rim does not appear anywhere before the middle 5th century, and I believe that the flat-topped hollowing of the base (to be contrasted with the watch-glass, or cupped, hollowing of the Irchester-Finningley-Chessel Down group) is another sign that we are advancing into the Dark Ages. There is a hanging-bowl from the Castle Yard in York, with an embossed silver print (fig. 3, A) inside and outside the base, that is probably of the same date as the Hawnby bowl; but, on the other hand, a bowl from Caistor in the Lincoln Museum may possibly be dated in the 5th century; it has circular escutcheons (fig. 3, B) bearing ring-and-dot patterns,

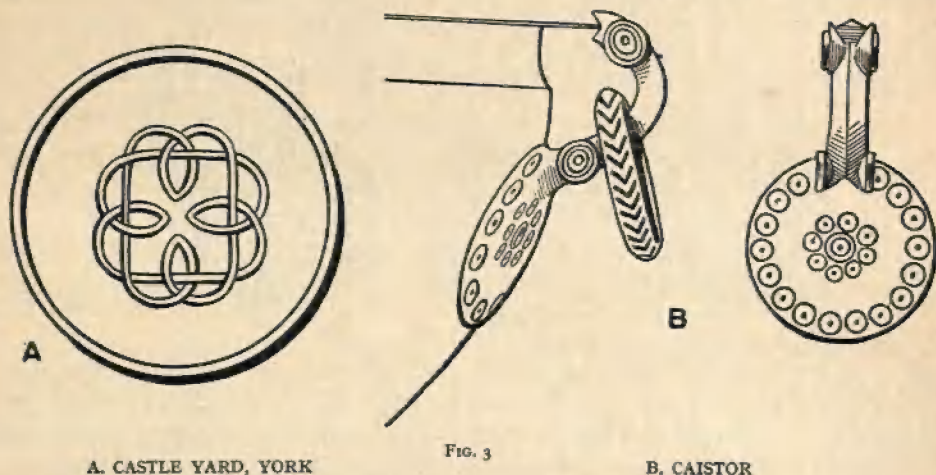
¹⁷ The hole had been plugged with lead.

¹⁸ On the other hand, some of the Norwegian non-enamelled 'heater' bowls may have been made in the 7th-9th centuries, *e.g.*, the Ulstein bowl in Bergen Museum.

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animal-headed hooks, and rather Roman-looking concentric rings of finely engraved lines on the underneath side of its base.

In England the series of hanging-bowls with plain (*i.e.*, not enamelled) heater-shaped escutcheons ends with vessels of the Hawaby type; but this does not bring the history of the heater escutcheon to a close, because this variety of hanging-attachment was subsequently made in Ireland, and there are several examples, sumptuously enamelled in the Irish enamel-style of the 8th and 9th century, that have been found in Viking graves abroad. I illustrate one of these later escutcheons that comes from Hoprekstad in western Norway (pl. VIII, 4). It belongs



to the same Irish school that produced the anthropomorphic enamelled handle-fittings of the kind that adorn the 'Buddha' bucket (pl. VIII, 3), found among the treasures in the Oseberg ship. We have yet to note, however, that at a much earlier period in England the heater escutcheon once or twice seems to have attracted the attention of the enamellers. Examples come from Faversham in Kent, Needham Market in Suffolk, and from Basingstoke. The escutcheons from the last two places belonged to bowls of the 6th or 7th century¹⁹; but the Faversham fittings (pl. v, 5) are much earlier, possibly 4th or 5th century. They consist of an escutcheon and a print, both decorated with a dainty little spiral pattern, and on the escutcheon you will see that three

¹⁹ The Basingstoke bowl has fish-tailed escutcheons and an extremely complicated intersecting circle device engraved on the base.

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of the spiral-terminals have 'eyes'; this design is closely connected with that on certain rare embossed disc-brooches of the Roman period and on a few Celtic 'latchets'. The zoomorphic terminals, best seen on one of the latchets, are, in my opinion, ultimately to be derived from the well-known Roman dragon-brooches; but you have to add a third head and arrange the brooch triskele-wise so that it fills a circle, in order to approach the latchet and disc-brooch design.

Before we pass to the hanging-bowls with escutcheons of the 'Enamelled Disc' class, we must say something about those that have 'Openwork Escutcheons'. There are only two examples, the Wilton bowl, and a vessel that is represented by a set of three escutcheons



FIG. 4. ENAMELLED ESCUTCHEONS FROM (A) NORTHUMBERLAND (B) BARLASTON (C) FAVERSHAM

found at Faversham. The Wilton bowl²⁰ is assuredly early, not later, I should think, than the beginning of the 5th century; it has a very narrow fold-over rim and four escutcheons (cf. the Sleaford and Lullingstone bowls), which are attached by rivetting²¹; they are not enamelled, and are decorated simply by a cruciform design in openwork, achieved by cutting out four pelta-shaped pieces from the field. The Faversham escutcheons (fig. 4, C) contain a Latin cross in openwork, flanked by two fish-like creatures with markings in red enamel; these may date from the 5th, or possibly 6th, century, and seem to me to show the influence of the church in France about the time of the conversion of Clovis. The Latin cross occurs several times in early Frankish (or late Gallo-Roman) antiquities, and is known also in

²⁰ Baldwin Brown, *Arts in Early England*, iv, pl. cxviii.

²¹ On the subject of rivetting and soldering (the usual method), see Mr Cowen, *op. cit.* p. 332.

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English cemeteries (*e.g.*, Chartham Down, Kent), so that it would be rash to say that it is exclusively late. Moreover, in this instance it may be regarded as a cross of the Wilton type that has only become Latin because this is demanded by the design of which it forms part. If you want to see this sort of openwork bronze casting, complete with cross and gaping animal-heads, abroad, you will find it on late Gallo-Roman antiquities in the Vermand cemetery.²² I may add, furthermore, that on the English evidence I regard the thick ribbed hook and the fiery little head of these mounts as being early.

The most decorative of the hanging-bowls are those fitted with escutcheons in the form of enamelled discs. One bowl (Benty Grange, see below, p. 178) stands apart from all the others in this class, but the rest can be provisionally grouped under three sub-headings, I—the 'Romanizing' Series; II—the 'Ultimate La Tène' Series; III—the 'Developed Trumpet-Pattern' Series.

I. The 'Romanizing' Bowls are of two kinds, one (A) having large and very crudely ornamented escutcheons, and the other (B) having small escutcheons that show much neater and better controlled enamel-work. An intermediate form (A-B) is provided by the well-known Lullingstone bowl, to which I shall refer presently.

The 'A' bowls are unfortunately represented only by escutcheons and prints, and by fragments of enamelled bronze strips that edged the base, and perhaps adorned the sides, of the now vanished vessels. There is a fine set from Dover (fig. 5) in the museum of that town, and there is also a set from Mildenhall in the Cambridge museum, both of which include enamelled prints from the bases of the bowls. The excessively coarse, but showy, enamel-work in these sets deserves careful examination; when you look at it closely (particularly one or two of the Dover discs) you will see that it is executed in the tradition of a recognized and famous school of Romano-British enamelling, the school that was producing at the beginning of the 2nd century such gaudy works as the West Lothian patera and the Bartlow Hills cup. You will notice, moreover, that these discs all bear travestied versions of classical patterns. Now constant features of the early pieces are a foliage belt of pairs of 'floating' leaves that are separated from their stalk, and a very pronounced toothing at the edges of the enamel fields (fig. 6); it seems to me that both these features occur in the Dover

²² See, for instance, *Mem. Soc. Acad. de St. Quentin*, 1888-9, series 4, IX, pl. 15, no. 11.

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escutcheons, where you have the floating leaves, either in pairs or singly, in the margin, and the toothed edging as a separate jagged and irregular ring inside. Not everyone, however, will agree about this ; but if we pass on to the ' B ' bowls, we shall find there is an example from Faversham in Kent (pl. III) with applied strips on its base and sides that reproduce really very closely this foliate border of the earlier



FIG. 5. 'ROMANIZING' ESCUTCHEONS AND PRINT, DOVER MUSEUM (†)

enamels (cf. the Braughing cup, pl. VII). When we add to this the fact that the Faversham bowl is of the simple Irchester type, we see that it may be possible to make out a case for dating some of these ' Romanizing ' bowls in the 4th or early 5th century. We must also note the prints of the Faversham bowl (fig. 7, 1) for these confirm the impression of early date ; one of them shows a frieze of little animals in the ' free ' style that appears on the continent in the 4th century, and the other a familiar Roman linked-loop device, here cut open and having the loose ends provided with animal heads. Two other bowls of the

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'B' kind come from Kent (Kingston cemetery), and I shall refer to them later on in connexion with the dating of the graves in which they were found (p. 182-3). They have folded rims and are probably a little later in date than the Faversham bowl; I illustrate one of them (fig. 8, A), and I will content myself with observing here that the enamel-work of their escutcheons is technically the same as that on many Roman enamelled disc-brooches, a remark which also applies to the escutcheons of the Faversham bowl.

Easily the most interesting bowl in this 'Romanizing' series is the famous vessel that was found at Lullingstone in Kent (pl. iv). It has four escutcheons, which we may on our present knowledge reasonably take to be an early feature; it is decorated with applied strips, which also seems to be a sign of early date in England; it has applied figures of animals, fishes, and birds, in the 'free' style (which remind us of the figures pinned on to the late Roman Thorsbjerg plaque);



FIG. 6. DETAIL OF ENAMELLED PATERA, LINLITHGOW

it bears a guilloche pattern such as might have been derived from Roman pavements, or from wire-work like that on the Rhayader enamelled bracelet; it shows a simple key-pattern exactly like that on the strip-work of the Mildenhall bowl; most of the enamelling is technically of the Dover-Mildenhall school, and we recognize this identical craftsmanship (so far as casting and finish are concerned), and some of the same patterns, in early continental work, particularly certain buckles. Everything, in fact, points to an early date for the Lullingstone bowl, except for the awkward circumstance that the escutcheons themselves bear a trumpet-pattern device that, ordinarily, would not be dated earlier than the 7th century. I think, however, that many of us have felt for some time that this pattern must have been used at a considerably earlier period; when we find it crystallized and perfect, a pattern familiar formula of the manuscript-illuminators of the middle 7th century (*e.g.*, the Book of Durrow), we can fairly assume that a considerable amount of experiment, first in metal-work and then in painting, must have preceded its appearance; yet we all know that it is extraordinarily difficult to trace it further back.

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I believe, however, that the constituent elements of the trumpet-pattern, as seen on the Lullingstone escutcheons, were known in late Roman Britain. Look at the pattern shown in fig. 9, left, which is the design on the inner panel of a well-known enamelled plaque of late Roman date that was found in the Thames, and set yourself the problem of converting this rectangular pelta-design into a form that will serve to decorate a circular space. You have got to arrange the peltas, with



FIG. 7. ESCUTCHEONS

(1) FAVERSHAM, (2) KINGSTON, (3) LULLINGSTONE, (4) BARRINGTON, (5) LOWBURY

All natural size, except 4 which is reduced

their interlocking expanding appendages, in a quite different way. Try with the two peltas that you have already, and you will not succeed. But take a third pelta; link up the ends of the three in the centre of your circle, and join up the attachments of each pair so that they reach towards the circumference of the circle; you will then find you have made a capital piece of 'Celtic' trumpet-pattern (fig. 9, right) directly out of a later Roman design. After this experiment, we are not going to believe that the trumpet-pattern enamels must necessarily be as late as the 7th century.

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I do not pretend that the trumpet-pattern of the escutcheons is directly derived from the ornament on the Thames plaque; on the contrary, I believe that Mr Romilly Allen was right in saying that it owes almost everything to embossed metal-work (and perhaps wood-carving) that we no longer possess. But if we look at the Lullingstone escutcheon (fig. 7, 3), we see that it does represent the 3-pelta arrangement that I have described, though the arms have been altered to fill up the space by the introduction of a 'metal' joint, and, consequently, one set of the little 'leaves' has got into the wrong place. We observe, also, that this design is executed throughout in coarse line; there are no fields of metal set off against the enamel, as in many of the better executed trumpet-pattern escutcheons, and I interpret this as a sign that the maker of the Lullingstone bowl was painfully copying a design that was still unfamiliar to him. He was, I feel, one of the first to make use of the trumpet-pattern in a circlet of champlevé enamel, and not a craftsman well used to this design.

For this reason I think that the Lullingstone bowl should take its place at the end of the 'Romanizing' series of hanging-bowls, which have escutcheons that show the inspiration of classical ornament, and I think that it should not be dated later than A.D. 500. Perhaps we should find that the Keythorpe bowl (now lost), which was of the same kind, but had only three escutcheons, belonged to the same series and was of about the same date. It will be observed that in this group there is next to nothing that we can describe as a contribution from 'Celtic' art. It is true that one of the Kingston bowls bears on the print a very pretty triskele (fig. 7, 2) drawn with the native feeling for curvilinear design; but this, after all, both in pattern and in the technical quality of the enamel, is not much of an advance on some of the enamelled disc-brooches bearing triskele designs that were made by Romano-British enamellers. You can see this same design, further developed, on the enamels of the newly discovered bowl from Stoke Golding in Leicestershire.

II. The second, or Ultimate La Tène, group of hanging-bowls (in this series with enamelled escutcheons) introduces us to craftsmen who were still resolutely Celtic and had no sympathy with Roman and contemporary continental design. The bowl from Barlaston in Staffordshire is the earliest and the most important of the vessels that are left to us from these workshops. In form it is of the simple Irchester type with in-turned rim, but it is only an imitation of the Irchester

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kind of bowl, because it is made of cast, not beaten, bronze.²³ It has a flat, wide foot-ring and an ornamental strip, and three large, clumsy escutcheons (pl. v, 1) bearing a rather limp, bedraggled curvilinear swastika, set off against a field of red enamel, with a coarse and badly fired piece of mosaic inlay at the centre (fig. 4, B). The decoration on

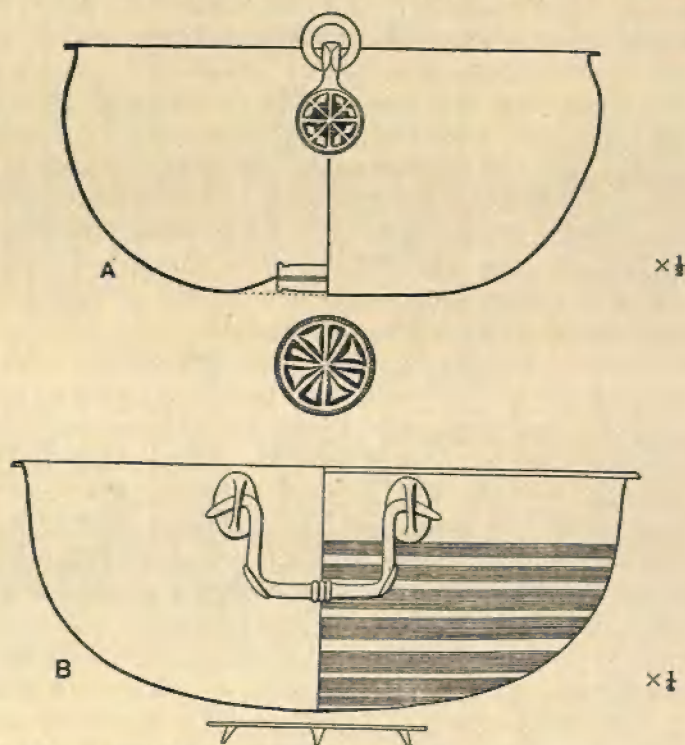


FIG. 8. BRONZE VESSELS FOUND TOGETHER, GRAVE 205, KINGSTON, KENT

the foot-ring is precisely the same as that on a piece of authentic 'Early British' enamelled metalwork, a terret from Bapchild in Kent (pl. v, 3), and technically the enamels of the Barlaston bowl belong to the Celtic school. Just the same sort of work can be seen on some of the Romano-British trumpet-brooches (pl. v, 2); it is distinguished by soft and heavy, thick-line, curvilinear patterns that are not in the least like the thin-line chisel-finished designs of the normal provincial Roman schools.

²³ On the difficult process of making these bowls see A. Götze, *Die altthuringische Funde von Weimar*. Berlin, 1912, p. 19.

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Three escutcheons and a print from a bowl, that was presumably found in Northumberland, give us another example, and I illustrate one of them (fig. 4, A). They all contain the same design, a miserable burlesque of a triskele, but the enamel fields in these plaques show not only mosaic inlay (hopelessly bungled), but also some creditable 'spotting' (*i.e.*, inlaid spots of another coloured enamel, introduced directly and without the employment of metal cloisons or cells). Both these tricks, spotting and mosaic, are due to Roman influence; we do not find them used in the Brigantian work represented by the trumpet-brooches and the dragon-brooches, and I imagine that our 'Ultimate La Tène' bowls represent the continuation of the native enamel-work of the north, some time after the break-up of the Brigantian schools,²⁴ at a period when the northern craftsman had become familiar with fashionable enamel-work and could attempt to imitate its most striking features. Nevertheless I cannot believe that the Barlaston bowl and the Northumberland escutcheons are much later than about A.D. 300.

An escutcheon from Utne in Norway (pl. v, 4), no doubt stolen from this country, shows us a simple and handsome swastika that furnishes us with yet another example of this 'Ultimate La Tène' enamel-work. There is also an escutcheon from Barrington in Cambridgeshire to be noticed (fig. 7, 4); it is to a certain extent a compromise between the northern thick-line and the Roman thin-line work, but the design, a flaccid, ill-drawn swastika, is unmistakably in the spirit of the school to which I am now inviting your attention.

III. The contrast between this work and the products of the school that produced the bowls with 'Developed Trumpet Pattern' escutcheons is very striking. One of these bowls, that from Lowbury camp in Berkshire, has a design on its escutcheons showing the development of the triskele, an antique motive, under the influence of the trumpet-pattern style (fig. 7, 5), but in my opinion the work of the new school is primarily concerned with modifications of late Roman pelta-trumpet patterns, like that which we noticed in connexion with the Lullingstone bowl. These patterns, however, were transformed with all the cunning and grace of which British art was master, and we have certainly travelled a long way from the original sources when we

²⁴ Probably about A.D. 150-60. On this see Mr R. G. Collingwood, *Archaeologia*, 1930, LXXX, 57.

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come to such beautiful pieces of work as the escutcheons of the Winchester bowl (pl. VI, 1 and 2) or the escutcheons that are to be seen in the Victoria and Albert Museum (pl. VI, 3 and 4). You will notice that the line-drawing of the Lullingstone escutcheon has disappeared, and that metal fields are cleverly introduced as a part of the pattern; this, I am inclined to think, marks the final development in the trumpet-pattern series. But before this is attained, a great many

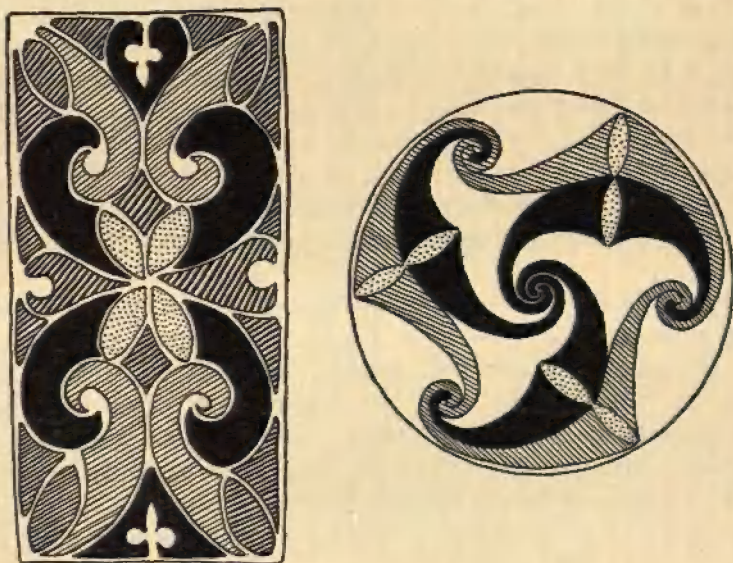


FIG. 9. ORNAMENT FROM ENAMELLED PLAQUE (left) ADAPTED FROM CIRCLE (right)

escutcheons had been made that are drawn in line only (fig. 10, A). Some of these (*e.g.*, Chesterton-on-Fosseway) are filled in with various coloured enamels, and some (*e.g.*, Oving) show the 'spotting' that we have already noticed as a Roman trick.

We do, in fact, see a very curious blending of ornaments and enamel-styles at about the time of the development of the trumpet-pattern; thus in the Chesterton-on-Fosseway group the print is a piece of thick-line enamelling and shows a form of the chain-pattern that we have already seen on the foot-ring of the Barlaston bowl; but the escutcheons are very creditable attempts at sophisticated thin-line work with trumpet-pattern. Again, in a Derbyshire barrow we find a trumpet-pattern escutcheon of considerable elegance accompanied

PLATE I



- 1-2. BOWL AND STRAINER FROM IRCHESTER, NORTHANTS. Diameter of bowl 11.5 inches. *Northampton Museum*
 3. SILVER VESSEL FROM TRAPRAIN LAW, HADDINGTONSHIRE. Diameter 12 inches. *National Museum, Edinburgh*
 4. BOWL FROM GRIND LOW, DERBYSHIRE. Diameter about 7 inches. *From a drawing in Sheffield Museum*
 5. BOWL FROM SLEAFORD, LINCOLNSHIRE. Diameter 12 inches. *British Museum*

PLATE II



1. BOWL FROM CHESSEL DOWN, ISLE OF WIGHT. Diameter 8.3 inches. *British Museum*
2. BOWL FROM SKOMROK, NORWAY. Diameter 18.3 inches. *Oslo Museum*

PLATE III



BOWL FROM FAVERSHAM, KENT, WITH DETAIL OF BASE. Diameter 7½ inches. *British Museum*

PLATE IV



THE LULLINGSTONE BOWL, WITH DETAIL BELOW. Diameter 10 inches. *British Museum*

PLATE V



1. ESCUTCHEONS AND FOOT-RING OF BOWL FROM BARLASTON, STAFFS. Slightly over $\frac{1}{2}$
2. ROMANO-BRITISH 'TRUMPET' BROOCH FROM CHEPSTOW, MON. Length 2.5 inches
3. LATE LA TÈNE TERRET FROM BAPCHILD, KENT. Length 3.3 inches
4. ESCUTCHEON FROM UTNE, NORWAY. Slightly reduced
5. PRINT AND ESCUTCHEON FROM FAVERSHAM, KENT. $\frac{3}{4}$

PLATE VI



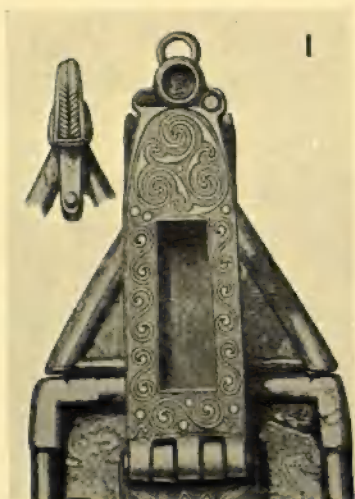
- 1-2. ESCUTCHEON AND PRINT OF THE WINCHESTER BOWL. *British Museum*
 3. ESCUTCHEON FROM HITCHIN, HERTS. $\frac{1}{4}$ *Victoria and Albert Museum*
 4. ESCUTCHEON FROM AN UNKNOWN PLACE. $\frac{1}{4}$ *Victoria and Albert Museum*

PLATE VII



1. ENAMELLED CUP FROM BRAUGHING, HERTS. Height, 2.2 inches. *British Museum*
2. THE WINCHESTER BOWL. Diameter 11.1 inches (for details see plate VI, 1-2). *British Museum*

PLATE VIII



1. DETAIL OF END VIEW, MONYMUSK RELIQUARY
2. ENAMELLED ORNAMENT FROM MIDDLETON MOOR, DERBYSHIRE. Length 1.5 inches. *Sheffield Museum*
3. ESCUTCHEON OF 'BUDDHA' BUCKET, FROM OSEBERG SHIP. $\frac{1}{2}$ *Oslo Museum*
4. ESCUTCHEON OF BOWL FROM HOPREKSTAD, NORWAY. $\frac{1}{2}$ *Bergen Museum*

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by a piece of enamel-work (pl. VIII, 2)—not, of course, necessarily of the same date—that shows a very interesting ‘thick-line’ experiment with the developed ‘metal field’ trumpet-pattern. This probably means that the thin-line work and the new pattern travelled from Kent or Wessex northwards in the early 6th century, and revolutionized the work of the craftsmen of the ‘Ultimate La Tène’ school. I regard the Derbyshire piece, which has an asymmetrical pattern on a red field and is unintelligently brightened by patches of yellow enamel, as north midland work of the 6th century.

When we look at the escutcheons of the Winchester bowl, we feel, I think, that no one but a Celt could have made these designs so beautiful, and could have drawn them with such ingenuity and delicacy; it is also well known that they became eventually the stock-in-trade of the Celtic illuminator, and confront us again and again in the pages of Celtic manuscripts. Yet when these designs first meet us in metalwork, that is to say in the bowl-escutcheons, they do not seem to me to represent a renaissance of an ancient Celtic art. On the contrary I regard them as an independent Celtic experiment with an entirely new design-complex, their Celtic quality being largely the genius that inspires the perfected patterns. If you want to study the fate of the antique Celtic art in late Roman and sub-Roman times you must turn back to the art which I have rather clumsily called ‘Ultimate La Tène’. In my view this is a phenomenon of the period A.D. 300-400; it produced embossed disc-brooches, the Barlaston bowls and its fellows, and the bird-ended spiral enamels of the heater-shaped Faversham escutcheon; it is an art that also survives on certain latchets in Ireland, and perhaps survives in that country and in Scotland until the 5th or 6th centuries, being represented by certain hand-pins and early penannular brooches. It is almost completely preoccupied with swastika and triskele designs, and it contains no elements not known in the 2nd century or before; every manifestation of it is simply the logical outcome of the faltering and uncertain development of native Celtic art that archaeologists have long ago traced as far forward as the 2nd century. But here, likewise, I do not talk about a renaissance; for nothing was reborn. We do not get back the beautiful foliate scroll of authentic La Tène art, or the billowing, sinuous eccentricity of the early masterpieces. When you look at the wretched leggy triskeles and swastikas, you can see for yourself that the old art is dying. That is why I describe it as ‘Ultimate’; because it is the end.

But this does not mean, as I have already observed, that Celtic art,

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in the sense of a national genius, is likewise extinguished. On the contrary, it lives with renewed vigour just because it was strong enough to abandon the antique *La Tène* formulae, and to occupy itself henceforward with the fascinating new trumpet-patterns, and the interlace, and animal composition that was borrowed from late Roman art. It is of this stuff, fresh and vigorous, that the Book of Durrow is born; it is of this stuff that we in England get our Winchester bowl.

To convince yourself that our enamellers were by this time really occupied with new motives, look at the remarkable bowl-escutcheon

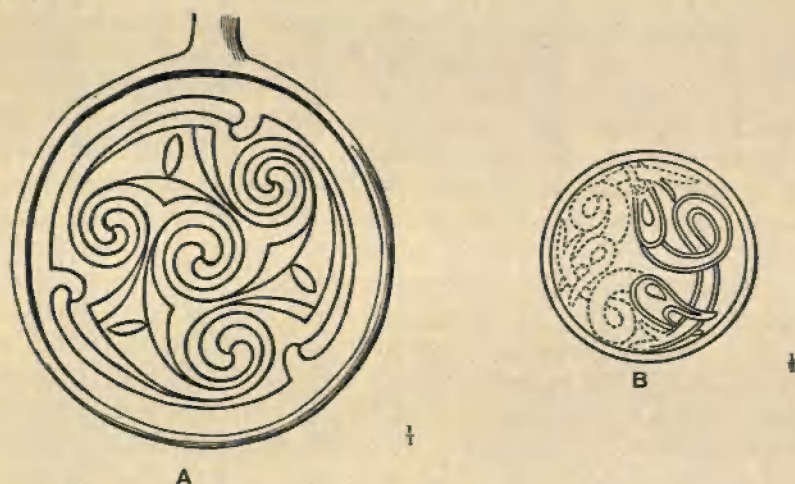


FIG. 10. ENAMELLED ESCUTCHEONS FROM (A) MIDDLETON MOOR (B) BENTY GRANGE, DERBYSHIRE

(silvered bronze and yellow enamel) that was found at Benty Grange in Derbyshire (fig. 10, B). It is a little roundel entirely filled by a design of animals biting each others' tails. This is probably a development of the animal-frieze that we saw on one of the prints of the early Faversham bowl (fig. 7, 1), and it is a design that we see further developed in the famous Celtic manuscripts. Originally it was not Celtic; now it has become characteristically so.

With this escutcheon and with the Winchester bowl we reach a date that must be in the neighbourhood of about A.D. 600, and we come to the end of the bowl series in England. This was the time that the Saxons, who ever since they had been in England had stolen British enamel-work with remorseless diligence, were becoming masters of luckless Wessex, and it may have been a result of their campaigns against

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south-central and western England that the native enamellers were driven into exile. Thus it comes about that we have nothing more to show from this country, whereas enamelling of the 'bowl-escutcheon' kind now appears in the Celtic refuge-lands and confronts us, for example, in that admirable (late 7th or 8th century) work of art, the Monymusk reliquary (pl. VIII, 1).

The part played by Ireland in the early history of enamel-work had been hitherto a small one. You do see it stated sometimes that Ireland preserved the native Celtic arts and crafts while the Romans possessed our island, and gave them back to us when the Romans left; but in reality there is not the slightest reason to suppose that this was so. Just what exactly are those who hold this view going to produce from Ireland in order to persuade us that there was a school of Celtic art there in the 3rd and 4th centuries so virile that it was able to influence late Roman or post-Roman Britain? There is, of course, next to nothing. You hear a great deal about a hiatus in Celtic art in this country during the last half of the Roman period, but this is a mere crack compared with the vast and horrible gulf that confronts you in Irish archaeology during these same centuries.

This is so evident that when we remember that we can produce quite respectable antecedents here for our late Roman enamels (some of which—such as the Dover escutcheons and the Kingston and Faversham bowl series—are definitely unlike anything ever produced in Ireland at all), and when we see from the distribution map (fig. 11) that the enamel-work in question is nearly all found west of the Fosseway, as far from Ireland as possible, we see that we need not waste time discussing the matter further. The stuff is clearly British in origin.

On the other hand, one or two Irish latches and a number of hand-pins (probably of Scottish origin) are the technical equals of some of our escutcheons, and must undoubtedly be ranked as a counterpart of British escutcheon-work at its best. But I am not inclined to date most of these antiquities earlier than the 5th century, and consider that up to the 7th century Irish enamel-work, in general, follows sorrowfully behind the British. You see this very clearly if you look at the early penannular brooches of 5th and 6th centuries, which show all manner of bungling experiments with mosaic inlay and extremely crude champlevé work. In the 7th century, however, when the Saxons had destroyed or nearly destroyed our British enamel-industry, then Ireland mothers this homeless craft and nurses it to an excellence

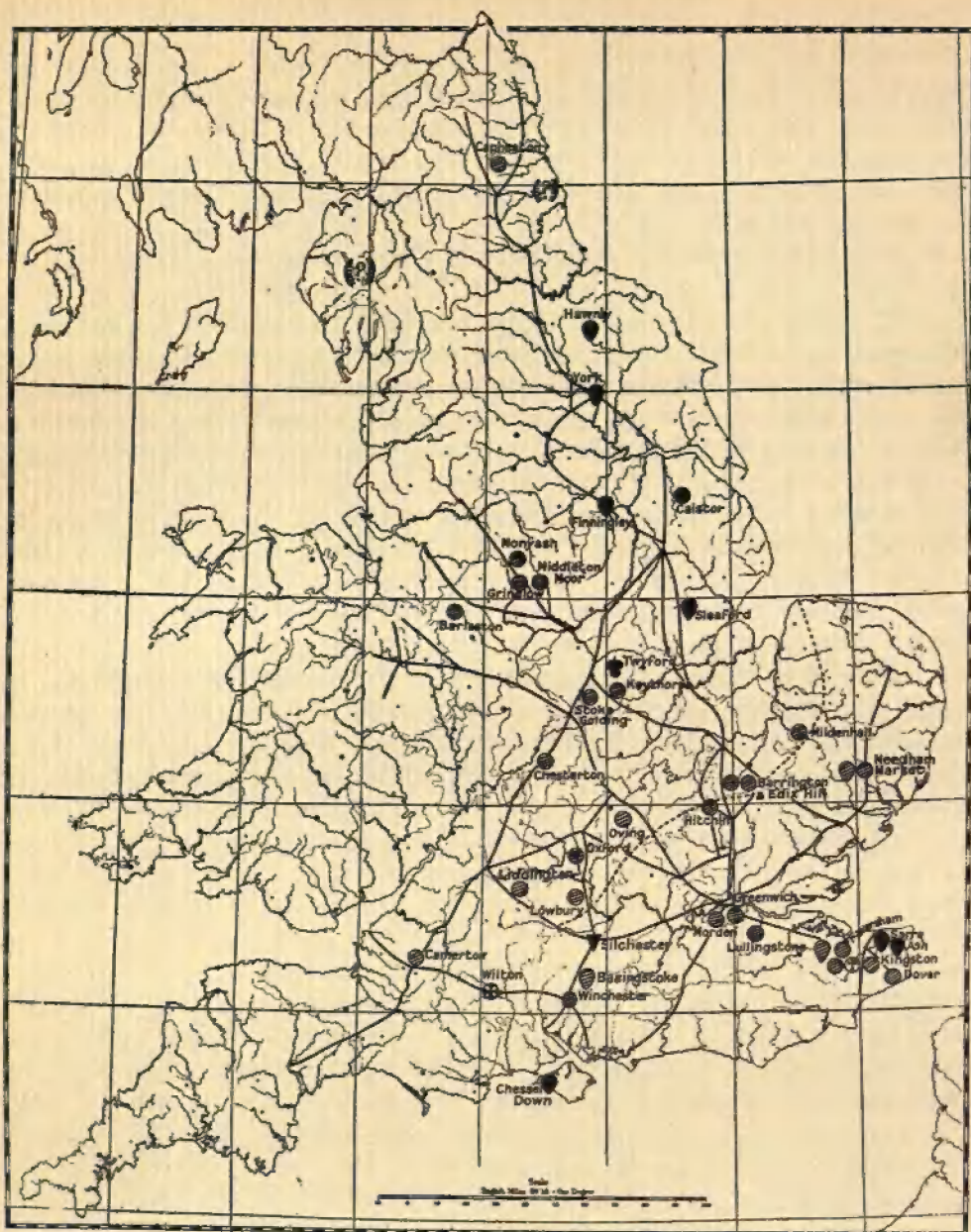


FIG. 11. MAP SHOWING DISTRIBUTION OF HANGING-BOWLS, AND THEIR RELATION TO ROMAN ROADS AND ICKNIELD WAY (dotted)

Hatched symbols represent enamelled bowls. Black symbols are unenamelled bowls. Shape of symbol represents shape of escutcheon.

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that it had never hitherto approached. The mosaic-work becomes fantastically brilliant and precise, a characteristically Irish false-cloisonné technique appears and the most audacious cloisonless polychrome work ever attempted in early enamel-craft is successfully achieved. The Tara brooch, the Ardagh chalice, the enamelled weights, and the work of the 'Buddha' bucket school, alike witness to the Irish enamellers' incomparable skill in the 8th and 9th centuries.

And now a word as to the Anglo-Saxons. I am sorry to say that I am not a believer in the high excellence of the pagan Saxon as an artist and a craftsman ; but, without seeking to defend this unworthy statement of opinion, I will merely remark here that, in my view, these Teuton invaders of Britain did not know anything about enamel-work—except that it was desirable stuff to steal. As far as I can discover, in the whole length and breadth of the lands from which the Saxons and the Angles came, there is only one object of antiquity which suggests that the inhabitants, at the time of the migration, knew anything about cooking fluxes in a muffle—and this is a poor little nielloed brooch which I should say myself was an imported piece. Therefore, when we find a few Anglo-Saxon long and square-headed brooches of the early 6th century in the Cambridge area ornamented with some simple champlévé blobs of red enamel, we ought to ask ourselves where in the whole world did the makers of these brooches learn the enamel-craft. And the answer is that they learnt it in Britain from the British.

It is to be admitted that the Jutes (or at least some of them) may have known a little about enamel-work before they came to our shores ; yet there is precious little evidence that they did. When you remember that in the vast collection of the metal-work that is alleged to have been made by them, we can only find one or two late saucer-brooches with a wipe of red enamel on their buttons,²⁵ you will see how very probable it is that the ancestors of the English, whether Angles, Saxons, or Jutes, made no contribution whatever to the furtherance of the enamel-industry that they found flourishing in this country.²⁶

²⁵ I omit the Ash brooch (a jewelled disc-brooch with an enamelled centre), as the enamel-work it bears is fine Frankish cloisonné.

²⁶ I do not want, however, to exclude the possibility that our enamellers in the south were to some extent influenced by the sub-Roman enamel-school of the Rhineland ; it seems to me that the 'fleur-de-lys' escutcheons of the Kingston bowl (fig. 8) may owe something to this influence.

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Yet the hanging-bowls of which I have been writing, are normally called 'Saxon Bowls'. It cannot be denied that many of them were found in pagan Saxon graves, and there is, in fact, only one little scrap of enamel-work of the escutcheon sort about which we can say for certain that it was *not* found in the possession of a dead Saxon, namely a fragment discovered close to Liddington Castle, a Wiltshire camp. Of course, there is doubt in many instances; we could not prove that the Chesterton-on-Fosseway bowl, or the Wilton bowl, or about 11 other bowls, were buried with Saxons, but we can say this was so, fairly confidently, of about 15 bowls, or parts of bowls. So it is not without reason that they have been called 'Saxon' and are supposed to have been made either by the Saxons themselves, or by British craftsmen under Saxon direction.

It is significant, however, that we do not always find a complete bowl in these Saxon graves; sometimes we find only an escutcheon or two, and in one instance, at the late cemetery of Camerton in Somerset, the escutcheons were clearly preserved as trinkets and worn by a child. This suggests 'loot' at once. But let us consider also the very important discovery of the Winchester bowl (pl. VII, 2). Everyone will agree that this lovely vessel is in magnificent condition; it must have been, in fact, almost new when it was buried. And it was found in the grave of a 7th century Saxon (he is dated by his dagger-pommel) who was buried in an earthwork outside Winchester. He could not have been at home, or near home, when he died; there is no Saxon cemetery, no settlement, within miles, so that he was plainly on a campaign. Which then is more likely—that he had this bowl made in order to carry it about, new and resplendent, to his battles—or that he had just stolen it, as I believe, from the still British town of Winchester?

I do not propose to say more on this subject, because, of course, I cannot prove my case; I can only repeat my conviction that all bowls, or parts of bowls, found in Teutonic graves got there as loot, and I would observe that this is no more astonishing than the admitted fact that the very numerous pieces of Irish metal-work found in Norway were not made by the men who last owned them. But what the reader may want to know, here and now, is whether the dates of these graves affect the chronology that I have suggested for the bowls. The answer is that they do not.

The most interesting of the graves in this connexion are the two burials in the Kingston cemetery (graves 76, 205) that contained the very early (as I think) 'Romanizing' bowls, one of which is illustrated

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here (fig. 8, A). The bowl that is not illustrated was found at the foot of a coffin that contained, beside the skeleton, an iron knife and dagger with a shell-encrusted silver pommel, and a simple bronze buckle; the bowl itself is worn and patched in more than one place, so that it was very old when it was buried. The second bowl is much better preserved, and I cannot pretend that it is necessarily much older than the burial in which it was found; it was found with a female skeleton, the beautiful Kingston brooch, a pair of one-piece silver brooches, a gold bracteate, an iron key, a pottery vessel and a glass cup, and the bowl itself lay inside a handled bronze pan of Roman type with a trivet stand (fig. 8, B). If you believe that the Kingston brooch was made in the 7th century I cannot hope there is any chance of convincing you that the hanging-bowl was made about A.D. 400. But if you believe, as I do, that the brooch was made about A.D. 500, you will see that there is no particular reason why the bowl should not be as early as I say. We cannot start a discussion here as to the date of the Kingston brooch; I merely beg the reader to fix his attention on those one-piece silver brooches, and to tell me where else one-piece silver brooches are found at this late day. He will answer in the Sackrau finds of the 4th century, and I am not going to believe they are accidental 1st century survivals, both at Kingston in Kent and at Sackrau in Germany; indeed, I myself think that they are quite sufficient in themselves to show that this famous Kentish grave is not much later than A.D. 500. As to the other graves in which hanging-bowls have been found, I have very little to say, except that what I take to be a rather late form of Saxon shield-boss was borne by the warrior who was buried with the Lowbury bowl, and that the graves containing the trumpet-pattern enamels seem, so far as we can tell at all, to be late (6th or 7th century).

To make an end of this matter, I believe that the hanging-bowls are from first to last British, and that the Saxons had no hand whatever in their manufacture. I think, moreover, that in origin these bowls are really Romano-British; that many of them had been made and were in use before the Romans left this country; that others were made after the Romans had gone, and belong to the almost unknown archaeology of the Arthurian period. The only subject on which, very regretfully, I find I have no opinion at all, is the problem of the use to which they were put. I take comfort, however, in the reflection that there does not seem to be any particular anxiety about the uses of the hanging-bowls found in Roman treasures, and I am content to accept our native hanging-bowls just as I accept the Traprain Law hanging-dish—or the

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little single-ring, jewelled cups (with interior prints) in the Szilágy-Somlyó treasure—without a vain struggle to decide what the owners kept therein.²⁷

I should like to say as a final word that this subject of the hanging-bowls, though in the broader aspect of their position in the history of Celtic art, is shortly to be discussed by my friend Mr E. T. Leeds. I think that on the general matter of the early (Romano-British) date of some of them Mr Leeds will endorse what I say here, but he tells me that he does not agree in certain particulars, and especially with regard to some observations that I have made about the development of Celtic art. I am not surprised ; indeed, when we reflect upon our lamentable ignorance of this subject, I think a divergence of opinion is not only natural, but healthy and desirable. So it only remains for me to admit honestly that Mr Leeds knows much more about Celtic art than I do, and, having done what I can in the rôle of advance-agent, I may without further remark safely leave the rest in his competent hands.

²⁷ See Mr Cowen's remarks, *op. cit.* p. 332ff.

A Chronological Table of Prehistory

by MILES BURKITT and V. GORDON CHILDE

WHEN the Editor of *ANTIQUITY* approached the authors and suggested that some of his readers would welcome a visual table showing the occurrence and sequence of the different prehistoric cultures, the matter did not seem to be one of outstanding difficulty. When the time came, however, to produce the work, it was found to be quite otherwise. Perhaps the ideal method would have been to prepare a series of gigantic distribution maps of the area to be covered (Europe, the Mediterranean Basin and adjacent regions) during the different prehistoric periods. But the preparation and publication of such maps on a scale large enough to be at all helpful would have presented insuperable difficulties. The more diagrammatic method which has been adopted divides up the map into several geographical areas which will be found heading the various columns of the table. These areas have been chosen and arranged as conveniently as possible in the light of the general distribution of the early and later prehistoric cultures.

It must always be remembered, however, that the spread of a culture from its area of origin corresponds to the ever-widening rings formed by a stone which is cast into a pond—its influence lessens as we get farther from the centre of diffusion. It is not easy to represent this lessening influence diagrammatically. Of course when the industries of a culture have spread widely over a neighbouring area, its name appears in the appropriate column of the table. Where, however, they are only very rarely found, perhaps as the last, faintest ripple of the circles, or where, as with the Solutrean culture in Spain south of the Pyrenees, they are only present in a small corner of the area adjacent to another thickly sprinkled area (France) whence they have clearly percolated, it would have presented an erroneous picture to have cited the culture-name.

A further difficulty presented itself when referring to the literature, namely the somewhat loose use by some authors of the prehistoric

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culture-names. For example, industries have been cited as Magdalenian because stratigraphically they were found later than Solutrean industries. It is now recognized that in many districts the Aurignacian culture continued to develop contemporaneously with, but quite independently of, the great Magdalenian culture which was essentially a French production. Care and judgment, therefore, have to be exercised in utilizing the literature of the subject—especially when this is not well illustrated.

It was found impracticable to give an absolute chronology much before 2000 B.C. In general terms, however, the reader may imagine that the Upper Palaeolithic period came to an end some 8,000 years ago and that its duration was in the neighbourhood of 10,000 years. Any attempt at absolute dating before the close of the Mousterian period is so hypothetical as to be useless. Nevertheless the antiquity of the human species on this globe may quite likely have extended into half a million years. Some prehistorians would call this a conservative estimate.

While the correlation of the several Palaeolithic cultures depends largely on geological considerations, and particularly on the succession of glaciations or correlative pluvials, it has been found inadvisable in view of the extreme uncertainty surrounding the subject at the moment to attempt to introduce into our table any detailed scheme of glacial chronology. In general it may be said that most of the Upper Palaeolithic period is anterior to Würm II; that most of the Mousterian is contemporary with Würm I; that the Acheulean can be correlated with the Riss-Würm interglacial; that the Chellean can be correlated with the next earlier interglacial stage (Mindel-Riss); and lastly that the Pre-Chellean culture seems to be definitely Pre-Mindel in age.

Though marked climatic and even geological changes can be detected in post-palaeolithic times, these do not as yet provide an equally delicate or comprehensive framework for the establishment of correlations between the several regions covered in this table. At the same time the variety of cultures increases bewilderingly. On the one hand the spread of forests and deserts caused the isolation of small groups which developed specialized cultures; on the other, when man learned to regularize his food-supply by the domestication of animals and the cultivation of grains, population multiplied rapidly and, being sedentary, gave rise to a multiplicity of distinct cultures. Though interrelations can certainly be detected between the various centres in these times,

A CHRONOLOGICAL TABLE OF PREHISTORY

they are in most cases so vague as to admit of the most divergent interpretations.

It seems highly probable that both food-production and then organized industry and commerce began first in Egypt or Mesopotamia or in their vicinity, and spread thence in very slow ripples so that Britain or Denmark might lag 2,000 or more years behind. About 3000 B.C. monuments dated by inscriptions begin both in Egypt and Mesopotamia, thus providing the framework of a historic chronology in terms of solar years. Yet even in those countries most dates for another whole millennium are disputed, so that even the correlations advanced here between early Egyptian and Mesopotamian cultures are speculative and not universally accepted. None the less one of the authors has ventured to set beside them some of the strictly prehistoric cultures of Europe and Hither Asia.

Owing to her regular contact with Egypt, Crete and rather later the rest of Greece can be brought into relation with the Egyptian record two millennia before written history begins there. Unambiguous synchronisms between the East Mediterranean and regions north of the Alps do not go back before 1200 B.C. Yet by that time a whole series of local cultures had been evolved in eastern, northern and western Europe. These can be arranged in a definite sequence, a relative chronology, in many areas, particularly those covered by columns 13-21, with a high degree of probability; and a number of very plausible synchronisms allow the several relative chronologies established for regions as far apart as Britain and south Russia to be coordinated into a coherent system. The position of the various phases in this system in relation to dated epochs in Egyptian and Mesopotamian history and so to our calendar reckoning is (it must again be insisted) much disputed. As to the beginning of the Bronze Age in Central Europe, for instance, opinions differ to the extent of 850 years, and in the case of the Neolithic period the range is much wider still. In the table a 'short chronology' has been adopted, but it embodies the views of only one group of writers. Hubert Schmidt for example would put most of the megalithic culture in Western Europe before 3000 B.C., and so parallel to the predynastic period in Egypt. Menghin on the other hand obtains a rather similar result by reducing the length of the predynastic period to 500 years. And even after 1200 B.C. there are possible discrepancies of at least a century plus or minus in nearly every column.

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SYSTEM OF NUMBERING THE NOTES

- | | |
|-------------------------------|---------------------------|
| 1. ICE AGES | 9. COPPER AND BRONZE AGES |
| 2. 'EOLITHIC' PERIOD | 10. IRON AGE |
| 3. LOWER PALAEOLITHIC PERIOD | 11. HALLSTATT PERIOD |
| 4. EARLIEST FLAKE-INDUSTRIES | 12. LA TÈNE PERIOD |
| 5. MIDDLE PALAEOLITHIC PERIOD | 13. MESOPOTAMIA |
| 6. UPPER PALAEOLITHIC PERIOD | 14. EGYPT |
| 7. MESOLITHIC PERIOD | 15. AEGEAN CULTURES |
| 7A. FOSSIL MAN | 16. BEAKERS |
| 8. NEOLITHIC PERIOD | 17. MEGALITHIC TOMBS |

1. ICE AGES

The Great Ice Age was contemporary with Quaternary or Pleistocene times. While it lasted long glaciers stretched out from all the mountainous lands of the northern hemisphere, while in the more northern latitudes there was a veritable ice-sheet over large areas. Nearer the equator pluvial periods took the place of the extremes of glaciation. There seem to have been four periods of glacial maxima, which have been named after four small rivers of southern Germany :—Gunz, Mindel, Riss and Würm. Between each of these came a period when the ice disappeared. The last of them, the Würm glaciation, can be divided into two periods of extreme cold, Würm I and Würm II,† there having been a recession of the ice in between. The cause of the glacial epoch is a much debated and undecided question.

2. EOLITHIC PERIOD

(*Eos*, dawn : *lithos*, stone).* A pre-palaeolithic stage in man's evolution. The evidence for its existence is based on the finding in Tertiary deposits of chipped flints, the chipping of which is accepted by the majority of prehistorians as being the work, not of natural forces but of some 'tool-making animal', presumably most primitive man.

The term 'eolith' is often incorrectly used in a contemptuous way to indicate a stone fractured by nature but vaguely recalling in shape some recognized type of tool.

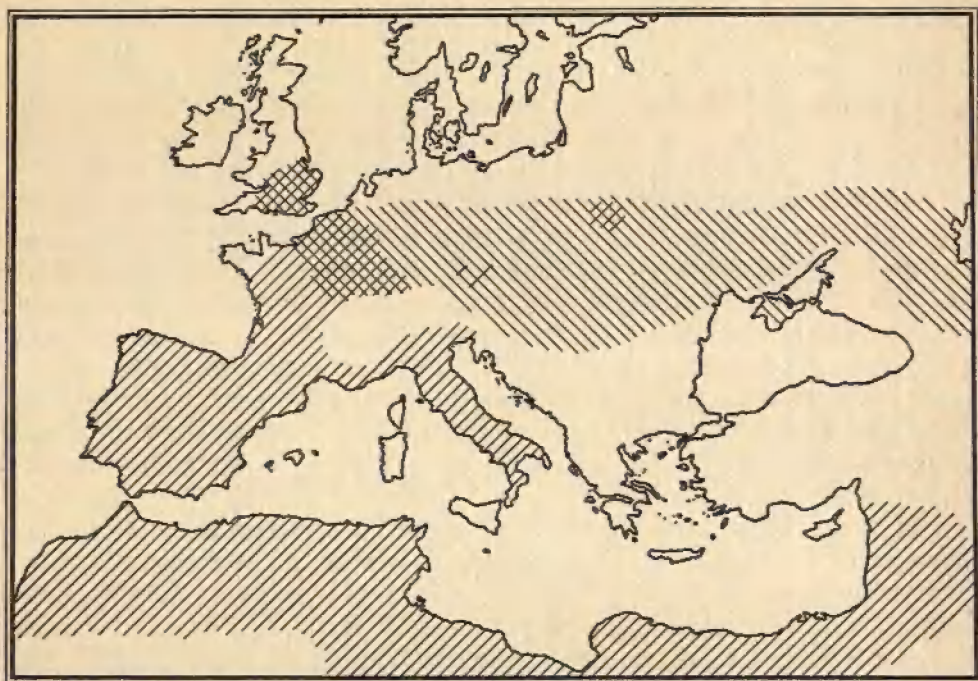
† Here used throughout as a synonym for the stage often called Buhl.

* The finds indicated by an asterisk in columns 11, 12 and 14 are not accepted as human artifacts by many prehistorians ; those from the Ipswich district in column 15 are accepted by the large majority.

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3. LOWER PALAEOLITHIC PERIOD

The Lower Palaeolithic Period (succeeding the Eolithic Period) lasted during the earlier two-thirds of Quaternary times. Though including the Clactonian and some of the Levalloisean cultures this was, above all, the period of the coup-de-poing in Western Europe. The industries are found in old river-gravels, brick-earths, early loess



Distribution of the Chellean and Acheulean /// cultures, and of the Pre-Mousterian flake-industries ~~~~ according to available data.

deposits, etc., and among these an evolution can be demonstrated on stratigraphical grounds to which two main names, derived from localities where the tools were first found, have been given: the CHELLEAN from Chelles on the river Marne, and the ACHEULEAN from St. Acheul, a suburb of Amiens. Both these can be again subdivided, the qualifying terms generally used being Lower, Middle and Upper or Late. The terms PRE-CHELLEAN and OLD CHELLEAN are sometimes used to designate the very early Chellean, and in Belgium certain industries of this date have been called STREPYAN. A very late and

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somewhat peculiar Acheulean is often named LA MICOQUE after a small rock-shelter in the Dordogne. A rather special Late Lower Palaeolithic occurring in North Africa has sometimes been named, after a type-station, S'baïkian.

4. EARLIEST FLAKE INDUSTRIES

Industries, the tools of which are made from flakes, are the special feature of the CLACTONIAN and LEVALLOISEAN cultures. In the case of the former culture the flakes are rough and show an unfacetted striking platform which, as a rule, is inclined at a high angle to the main flake-surface. The Levalloisean flake-tools, however, are often well made: they show much primary working on the upper surface and sometimes secondary working round the edge, the platform is at right-angles to the main flake-surface and it is very often carefully prepared by facetting. The process of tool-making by the technique of the tortoise-core and Levallois flake first makes its appearance at this time. It consists in the preparation of the upper surface of the tool while it is still a part of the core and before it is finally struck off by a single blow; so that only a little secondary working round the edge is then necessary to complete the flake-tool. These flake industries seem to be quite distinct from the coup-de-poing industries, although often found associated and in large measure contemporary with them in Western Europe.

If the original makers of these PRE-MOUSTERIAN flake industries differentiated from the same stock as Lower Palaeolithic man the differentiation must have taken place in some area outside Western Europe, where we only know the tools in a completely developed state and can trace in them no evolution from any other tool-form. The relationship between the PROTO-MOUSTERIAN in Central Europe and these flake cultures is doubtless close, probably a direct evolution. A rough industry first recognized in Belgium and called MESVINIAN belongs to this group of cultures.

5. MIDDLE PALAEOLITHIC PERIOD

The Middle Palaeolithic Period embraces only one culture, the MOUSTERIAN, so named after a cave at Le Moustier in the Dordogne region, southern France. The true Mousterian industries are characterized by flake-tools such as side-scrapers, points and Levallois flakes. The secondary working of the tools most often shows step-chipping. To be distinguished from the flake industries which are contemporary

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with the Lower Palaeolithic coup-de-poing cultures, they are found associated with an arctic fauna and were made by men of the NEANDERTHAL race. In certain areas such as Central Europe Mousterian-like industries which appear to be of somewhat earlier date do occur, but these are usually classed as Pre- or Proto-Mousterian. Some small,



Distribution of the Mousterian ||||| and Aterian == cultures, according to available data.

triangular, coups-de-poing survive into early Mousterian times from the previous Acheulean culture ; they are known as the COMBE-CAPELLE type of coup-de-poing. In North Africa a rather peculiar Mousterian is called ATÉRIAN ; among other tool-types there a sort of tanged Mousterian point is prominent.

6. UPPER PALAEOLITHIC PERIOD

It was with the beginning of Upper Palaeolithic times that *Homo Sapiens* crossed from North Africa into Western Europe. There seem to have been, from the beginning, several variations of a hypothetical,

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basal, physical type which have been named CRO-MAGNON, COMBE-CAPELLE and GRIMALDI after localities where human remains have been unearthed. The basal culture of these NEANTHROPIC people was the AURIGNACIAN, so-named after a site now destroyed near Aurignac in South France. In France the Aurignacian culture is divisible into



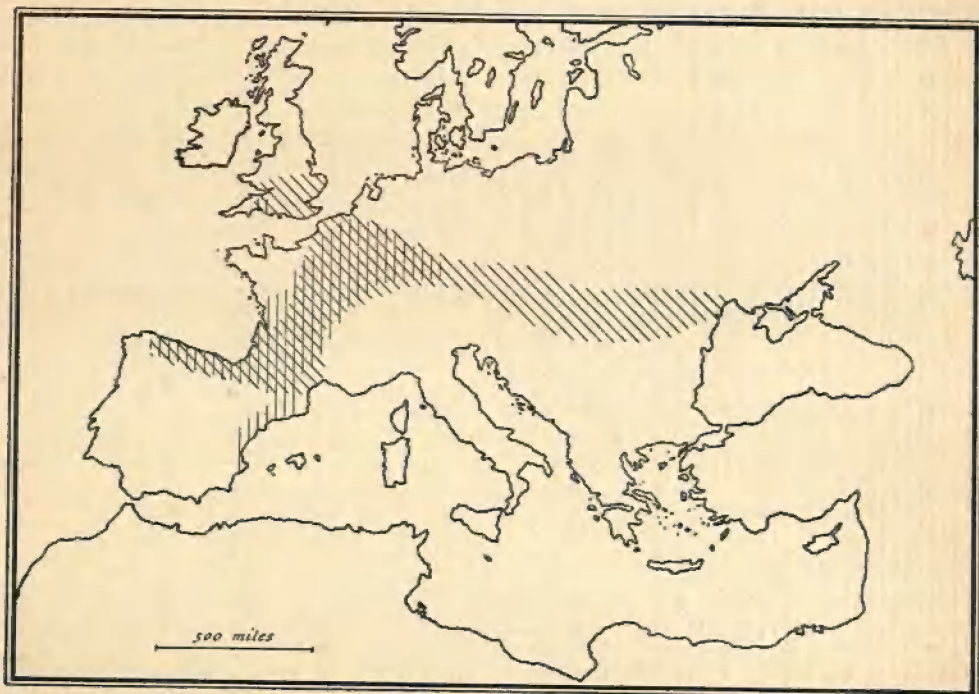
*Distribution of the Aurignacian culture, according to available data.
In N. Africa & in most of the Spanish peninsula the culture marked as Aurignacian is actually of the Capsian variety.*

five stages: the AUDI, the CHÂTELPERRON, the MIDDLE AURIGNACIAN, the GRAVETTE and the FONT ROBERT stages. The industries found in each of these stages include characteristic tools.

A little later there developed—probably in North Hungary—a new culture called the SOLUTREAN after a type-site in Eastern France. The industries show new types of tools, particularly the laurel-leaf spear-head, often made by a new technique of pressure flaking. The Solutreans invaded parts of Western Europe and dominated the Aurignacians. Three stages of their culture, known as Proto-Solutrean, Typical Solutrean and Late Solutrean, can be distinguished.

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Later the culture became extinct or was merged in the rise of the MAGDALENIAN culture of France, which seems largely to have had its origin in a renaissance of the earlier Aurignacian. The Magdalenians were, for the most part, the authors of the wonderful works of art which are found in the caves and rock-shelter homes. In many areas, however,



Distribution of the Solutrean\\\\\\ & Magdalenian|||| cultures, according to available data.

the Aurignacian culture was not disturbed by the Solutreans and Magdalenians, and in consequence it continued to develop independently of them. Various names, more particularly CRESWELLIAN and GRIMALDIAN, have been given to this 'developed' Aurignacian, which was contemporary with but untouched by the Solutrean and Magdalenian of France. In North Africa the Upper Palaeolithic industries are known as CAPSIAN (Early and Late, or GETULIAN). In the Nile Valley some rather peculiar industries, probably allied to the Aurignacian, are called SÉBILIAN. Sudden climatic changes probably caused the final break-up of the Upper Palaeolithic Civilization.

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7. MESOLITHIC PERIOD

The term Mesolithic is used to designate a number of cultures which flourished during that rather dim period between the end of Upper Palaeolithic times and the appearance of the true Neolithic civilization. It embraces an earlier group of cultures: the AZILIAN (typical stag's antler harpoon and painted pebbles), TARDENOISIAN (pigmy geometric tools), MAGLEMOSEAN (typical harpoon) later developing into the KUNDA culture, CHWALIBOGOWICIAN = SWIDERIAN (pigmy tools), and also a rather later group comprising the CAMPIGNIAN (typical axe), the ERTEBØLLE, or KITCHEN MIDDEN, and NØSTVET cultures of the Baltic area which are really also Campignian, and the ASTURIAN (typical pick). The Mesolithic cultures in Europe seem to have had two main sources of origin: (1) the final developments of the native Capsian or Aurignacian cultures by way of such industries as the Grimaldian and Creswellian and (2) the Campignian culture which seems to have penetrated into Europe from the east. In the hinterland of Scandinavia, etc., the Mesolithic cultures of the Baltic area seem to have survived for a long time. This late development is called the ARCTIC culture, of which there were several local facies. In Palestine Mesolithic industries are called NATUFIAN. There agriculture was practised but connexions seem mainly to have been with the north, not with Egypt. In date the Natufian is earlier than Middle Pre-dynastic times.

7A. FOSSIL MAN

Well authenticated definitely dated finds of prehistoric human remains are not numerous. The following are the more important discoveries arranged under chronological headings:—

PRE-MOUSTERIAN discoveries not belonging to the Coup de Poing Culture	{	PILTDOWN MAN (England)
		PITHECANTHROPUS (Java)
		SINANTHROPUS (China)
		EHRINGS DORF (Germany)
		TAUBACH (Germany)
		HEIDELBERG (Germany)
COUP DE POING MAKERS	..	None yet found
	{	NEANDERTHAL (Germany)
		SPY (Belgium)
		CHAPELLE AUX SAINTS (France)
		LA QUINA (France)
MOUSTERIAN MAN	{	GIBRALTAR
		LA NAULETTE (Belgium)
		KRAPINA (Yugo-Slavia)
		LA FERRASSIE (France)
		GALILEE and MT. CARMEL (Palestine)

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AURIGNACIAN MAN	{	PAVILAND (Wales)
		CRO-MAGNON (France)
		COMBE CAPELLE* (France)
		MENTONE (France)
		MENTONE ('GRIMALDI INFANTS')* (France)
		BRNO* (Moravia)
<p>* These remains cannot be classed as belonging to individuals of the Cro-Magnon type, but nevertheless they appear to be makers of Aurignacian industries.</p>		
SOLUTREAN MAN	{	LE ROC (France)
		KLAUSE (Germany)
MAGDALENIAN MAN	{	CHANCELADE (France)
		LAUGERIE BASSE (France)
		PLACARD (France)
		SORDES (France)
		LES HÔTEAUX (France)
		OBERCASSEL (Germany)
MESOLITHIC FOLK†	{	OFNET (Bavaria)
† From this time onwards many mixed types occur.		FURFOOZ (Belgium)
		AXPEA, near VITTORIA (Spain), etc.

8. NEOLITHIC PERIOD

The so-called Neolithic Period is generally supposed to be defined by the use of celts (axe or adze-heads) of stone with their edges sharpened by grinding. The makers of these tools were moreover generally food-producers—*i.e.*, they possessed domestic animals, cattle, sheep and swine, and/or cultivated plants (generally wheat and barley); they generally made pottery and often practised weaving too. But there are cultures, *e.g.*, the Dwelling-Place (Arctic or Comb-ware) cultures of Sweden and Finland, that, though characterized by polished stone celts and pottery, yet lack agriculture or domestic animals other than the dog. (The horse seems to have been domesticated in the Neolithic culture of South Russia and Denmark, perhaps also of Great Britain). Conversely some food-producing peoples like the pre-dynastic Egyptians did not manufacture polished stone celts. Here the term is applied to cultures without metal tools but possessing polished stone implements, pottery, agriculture, and tame food-animals, or at least two of those traits. The really essential feature of the community thus characterized is its self-sufficiency: it did not depend on external trade for any essential industrial material.

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In any case it must be remembered that neolithic cultures do not begin nor end at the same time everywhere. The Maori, for instance, were in a pure neolithic stage when Captain Cook landed in New Zealand. Moreover stone celts continued to be used and very probably to be made long after the introduction of metals locally ; in Scotland, for example, they are found in Iron Age sites.

9. COPPER AND BRONZE AGES

The discovery that certain minerals on heating with charcoal yield a substance which is fusible and malleable when hot, but, when cold, possesses the trenchant properties of stone, marks a real stage in human progress owing to the greater command over Nature which followed and also owing to economic adjustments, *viz.*, specialization of labour and the establishment of regular trade, normally involved. A new era is accordingly denoted by the regular use of implements made of cast metal. On the other hand neither objects made of native copper treated like a stone and fashioned by hammering when cold, nor yet occasional imported trinkets of metal denote the beginning of a metal age. In any case the metal ages begin at different times in different places according to their distance from the original centre of discovery, presumably in the 'Ancient East', and to their resources in ore or in commodities that could be exchanged for metal.

The use of unalloyed copper probably preceded that of bronze, an alloy of copper and tin which is easier to cast than the unalloyed copper. At the same time the use of the alloy was dependent upon the possibility of obtaining the comparatively rare element, tin, and would accordingly vary greatly with economic and geographical circumstances. The Sumerians used bronze more freely in the fourth than in the third millennium B.C., presumably owing to the depletion of the original source of tin supply or the interruption of communications. The presence or absence of tin in a collection of implements is rather an indication of the economic resources of their makers than of their age. In the index 'Copper Age' denotes an early metal-using culture in certain regions where such a culture using unalloyed copper is succeeded by one using bronze, the fact being established by a sufficient number of analyses. In the East Mediterranean, however, the term Bronze Age is applied, in accordance with traditional usage, also to cultures where the extant analyses suggest at least a shortage of tin. It must again be insisted that it is the presence of cutting tools and

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weapons of copper or bronze, not the absence of similar stone implements, that denotes a metal age.

In parts of Europe it is possible to divide the Bronze Age into three phases, Early, Middle and Late, characterized respectively by flat triangular daggers and flat or slightly flanged celts ; by rapiers and flanged or winged celts or palstaves ; and by cut-and-thrust swords, socketed celts or winged palstaves. But some of these types denote a culture (the fashion of a particular community) rather than a period of time, and they are in general only helpful at all as chronological criteria within the framework of the Central European-Scandinavian economic system.

10. IRON AGE

Iron is a much commoner element than copper, to say nothing of tin. The invention of an effective process for working iron accordingly introduced an industrial revolution, in that it made metal tools cheap and accessible for all purposes. The Iron Age begins then at different times as this secret spread from the centre of discovery in Hittite Asia Minor during the 14th century B.C., reaching Southern England, for instance, first toward the sixth century. The occurrence of stray ornaments or even implements of meteoric iron in Egypt and Mesopotamia before 3000 B.C. does not denote the beginning of an iron age.

11. HALLSTATT PERIOD

A settlement and rich cemetery of salt-miners at HALLSTATT in Upper Austria has given its name to the cultures of the FIRST IRON AGE in Central and Western Europe. The cultures so designated differ quite widely amongst themselves, being mostly direct descendants of distinct cultures occupying the several regions during the Late Bronze Age. All, however, share certain types of sword and later of safety pin and certain tendencies in pottery ; but it is incorrect to speak of a single 'Hallstatt culture'. The similarities seem due partly to common cultural borrowings from Italy and partly to the relationship between the parent Bronze Age cultures. A fusion between the two great groups of the Later Bronze Age—tumulus-builders and urnfield-folk is, however, a mark of the period in several regions.

Some divergence exists as to the precise delimitation of the Hallstatt period. Prof. Reinecke assigns to it a group of urnfields which, though directly ancestral to certain Hallstatt groups, lack iron and have no

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exact counterparts at Hallstatt itself. Here his 'Hallstatt A' is treated as a phase of the Bronze Age.

12. LA TÈNE

A trading post on Lake Neuchâtel in Switzerland gives its name to certain cultures of the SECOND IRON AGE. These, despite very marked local divergences, are still more homogeneous than those grouped under the name Hallstatt, and are indeed all facies or hybrid versions of single culture. This took its rise in eastern France, Switzerland or southwest Germany under Greek inspiration, transmitted principally from the Greek colony of Massilia (Marseilles) but partly also through Etruria.

It seems most likely that the diffusion of this culture was due almost entirely to racial movement rather than cultural borrowing. The authors and carriers of the cultures were admittedly Kelts but there may have been Kelts without a La Tène culture, while some of the later La Tène cultures belong to Kelticized peoples rather than Kelts.

13. MESOPOTAMIA

Mesopotamia was first unified by a Semitic king of Babylon, Hammurabi, shortly after 2000 B.C. Previously independent dynasts had ruled over various City-States. Lists compiled shortly before 2000 B.C. purport to give the names and regnal years of dynasts who had obtained the hegemony over the whole land since an event termed the FLOOD, and name also kings who ruled before it. We should thus have a complete historical chronology reaching back to prediluvian times, were it not that the lists give in some cases obviously fabulous figures, and in others represent as ruling consecutively dynasts who actually ruled side by side. The earliest kings mentioned in the lists, actual monuments of whom survive, belong to the 1ST DYNASTY OF UR, the third after the Flood. It is generally dated about 3000 B.C., though some authorities would reduce these figures by 3 or 4 centuries. Under, and presumably older than, the deposits of Dynasty I at Ur are the Royal Tombs and other remains of a highly developed urban civilization, already with written records. Parallels have been found also at Kish and elsewhere. Still earlier are deposits of a different civilization already with writing and characterized by painted pottery best represented in the ruins of JEMDET NASR. The Jemdet Nasr culture is preceded at URUK (Warka), Ur and elsewhere by an older urban civilization in which writing was being developed, that is termed the URUK I culture. Still older is a culture represented by a fine painted pottery akin to that widely diffused

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on the Iranian plateau, with which metal objects are sometimes already associated. The culture has been named after AL'UBAID, a small village near Ur, where it was first identified. At Ur the characteristic pottery is found below as well as above fluvial deposits attributed to the traditional FLOOD.

14. EGYPT

The Nile valley was reduced to a single kingdom a little before 3000 B.C. by a king of Upper Egypt traditionally known as Menes. Thereafter the land was ruled by a series of Dynasties of varying power, three main epochs of complete internal solidarity and colonial expansion abroad (termed the Old, Middle, and New Kingdoms) being separated and followed by periods of decadence, internal anarchy and foreign invasion (1st Intermediate period, Hyksôs conquest, etc.). The growth of Egyptian culture before the reign of Menes is known by a large series of cemeteries (chiefly in Middle and Upper Egypt) that are called predynastic. The length of the predynastic period can only be guessed (I regard 2000 years as a minimum estimate). Throughout the period we are dealing with settled people, burying their dead in regular cemeteries, cultivating wheat and barley and possessed of domestic cattle, sheep and pigs. Polished stone celts are limited to the earliest phase, and small objects of copper soon occur, but there are no signs of true metallurgy before the Gerzean or Middle Predynastic, and only the Late Predynastic can be called Copper Age.

15. AEGEAN CULTURES

The prehistoric metal-using cultures of the Aegean fall into three interrelated groups—MINOAN in Crete, CYCLADIC in the Aegean Islands, and HELLADIC in Mainland Greece and on some of the western isles (Levkas and Thiaki). The Minoan culture (so termed by Sir Arthur Evans after Minos, a legendary king of Knossos in Crete) was throughout an urban civilization with regular trade and specialization of crafts and industries. Its history is divided into three main periods, Early, Middle and Late, each subdivided into three. The same general divisions are applicable to the humbler Cycladic and Helladic cultures. In Late Aegean times the Minoan culture spread to Mainland Greece and the Islands, developing in the former area a variant termed Mycenaean. In Late Aegean III, about 1400 B.C., this mainland offshoot outstripped its parent and became the dominant culture throughout the Aegean area.

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16. BEAKER

This term (sometimes with the qualification 'Bell-beaker') is technically restricted to a class of beaker-shaped vases, decorated with patterns arranged in horizontal bands often executed with a short-toothed comb, that are found in graves, generally with contracted skeletons of brachycephalic type. They are often associated with flat, rivetless, tanged daggers, awls of copper, buttons with v-perforation, gold trinkets and (except in Spain) bowman's wrist-guards. These peculiar vessels are found in megalithic tombs in the Iberian Peninsula, South France and Brittany, and in rock-cut tombs in Sardinia and Portugal, but in trench graves without barrows in Upper Italy, Bavaria, the Rhineland, Thuringia, Silesia, Czechoslovakia, Austria, Hungary and Galicia. Two, apparently imported from Bohemia, have been found in Danish Passage Graves (dated to phase III there).

Analogous vases are found in the earliest round barrows of Britain, marking the beginning of the Bronze Age here. Those assigned to class B approximate closely to continental bell-beakers, especially of the Rhinish variety, and are associated with similar grave-goods. The British types A and C resemble rather a hybrid form found in Holland and southwest Germany and supposed to be influenced by Corded Ware. Like the latter, they may be accompanied by battle-axes of stone.

The bell-beaker typifies a culture which spread very rapidly over a great part of Europe, most authorities say from Spain. Being generally short-lived outside Spain and Britain, beaker-burials are only sporadic; they therefore form a useful chronological horizon in many columns of our table. It is generally assumed that the beaker-folk settled in Britain about the same time as they overran Central Europe, but that here they continued to manufacture beakers long after they had vanished elsewhere.

17. MEGALITHIC TOMBS

Megalithic (*μεγας*: great; *λιθος*: a stone) tombs are usually divided into dolmens, passage graves and long cists. Formally a DOLMEN is a large box or small chamber formed by three (or four) very large stone uprights (pillars) supporting a gigantic capstone. Dolmens seem in all cases to have been buried in a mound up to the top of the pillar stones. Monuments corresponding to this definition occur in northern and western Europe, North and Central Africa,

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Sardinia, Galilee and Transjordan, South Russia, Persia, India, etc., and may be of very varied age not necessarily particularly ancient. In Denmark, however, such tombs are the earliest megalithic monuments and give their name to the first clearly defined phase of neolithic culture there. The Danish nomenclature must be extended to other regions, but evidence does not, however, prove that all apparently similar structures are equally ancient.

PASSAGE GRAVES are chambers built in the manner of dolmens but of larger size and preceded by an entrance-passage leading to the chamber. Such are known especially in Portugal, North Spain, Brittany, England, Wales, Denmark and northwest Germany. In Denmark they belong as a whole to a period later than the dolmens. Finally STONE CISTS are long chambers (often subterranean) not preceded by a passage but opening through a portal onto a shallow porch. Tombs of this type occur in Sardinia, the Iberian Peninsula, all over France, in Great Britain, Ireland, Denmark, South Sweden and Central Germany. A direct evolution from the simple dolmen to the passage grave followed by a degeneration to the cist was once generally postulated but is now a discredited doctrine. All three forms are rather supposed to be versions of rock-cut chamber-tombs such as occur in Egypt, Syria, South Russia, Galicia, the Aegean, Sicily and Malta, as well as in Portugal, Sardinia and France. The so-called 'tholos tombs', beehive chambers roofed by corbelling and generally entered by a passage, such as occur in the Aegean, southwest Spain, South Portugal, Ireland and North Scotland may be treated as intermediate between rock-cut chambers and megalithic-built passage graves. It must be remembered that chamber-tombs continued to be built or cut down to quite recent times in the East Mediterranean area though the practice goes back at least to the third millennium there.

NOTE. Many of the places which are mentioned in the above notes and in the Chronological Table will be found on the maps illustrating Professor Gordon Childe's books, *The Dawn of European Civilization* (Kegan Paul, 1925); *The Danube in Prehistory* (Oxford University Press, 1929); *The Most Ancient East* (Kegan Paul, 1928); *The Bronze Age* (Cambridge University Press, 1930).

An INDEX to the TABLE and the NOTES is printed on the following pages.

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Chronology of Prehistoric Europe : a Review*

by V. GORDON CHILDE

[As an appropriate addition to the introduction to the Chronological Table which precedes this article we print Professor Gordon Childe's review of Dr Åberg's important work on Bronze Age chronology.—EDITOR].

MOST of us have been nourished on text-books which placed the beginning of the New Stone Age in northern Europe seven or eight thousand years ago. Such figures are of course just guesses. Though de Geer's geochronology would seem to lend them a semblance of scientific authority, the recent tendency among archaeologists, particularly in this country, has been to reduce them very materially. Dr Åberg's latest book represents almost the culminating point in this tendency. The earliest tangible manifestations of neolithic culture in northern and central Europe—the Danish dolmens and the Danubian I culture—are here brought down nearly to 2000 B.C., to the Middle Minoan Age of Crete, the time of the Egyptian Middle Kingdom and the final decay of Sumerian civilization. The reasons which have led Åberg to these really revolutionary conclusions are not fully stated in the present volume : they depend in part upon the results of his studies on the Italian Iron Age and the Hallstatt period, already published and reviewed in *ANTIQUITY*¹ ; on the other hand a fresh analysis of the continental Bronze Age is promised which will link up those periods with the Copper and Early Bronze Ages here studied, while Aegean chronology is to be subjected to an annihilating criticism. The work will eventually offer a single self-contained system the validity of which will largely be guaranteed by the internal coherence of the whole. No final judgment can then be passed on any isolated section.

* *Bronzezeitliche und früheisenzeitliche Chronologie*. III, *Kupfer- und Frühbronzezeit*. By Nils Åberg. Stockholm, [Kungl. Vitterhets historie och Antikvitets Akademien] 1932. 25 Kroner.

¹ March 1931, p. 125 and September, p. 385.

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Nevertheless the present work challenges a re-examination of the methods and data upon which the chronology of prehistoric Europe depends. Now the relative chronology of prehistoric times is determined principally by two methods—stratigraphy and typology. Where the continuous occupation of a single site over many generations offers us a complete picture of the developments and fluctuations of the local industries, the data of stratigraphy provide an indisputable standard. The inhabitants of Cisalpine Europe, however, from the New Stone Age onwards were distinguished by the peculiarity noted by Strabo 'of living in huts and readily shifting their settlements'. And so we lack stratified sites continuously occupied over a long period. On the contrary, our barbarian ancestors were continually introducing improvements into their tools and weapons partly perhaps, as Åberg hints, just because of the instability of their habits, partly because the models were often taken over from alien civilizations and had to be adjusted to local needs. Thus it becomes easy to establish chronological series illustrating the progressive development of axes, daggers, pins and razors.

Dr Åberg handles the typological method with masterly judgment and has purified his series from the local variations which in other systems masquerade as symbols of periods. But, like other exponents of the method, he is obviously unhappy when he comes to regions, like Mesopotamia, Crete, or even Hungary, where among the articles best suited to typological arrangement—metal tools, weapons and ornaments—satisfactory types had been early created and were thereafter retained without material modification. The stratigraphy of the *tells* in those countries attests the persistence of certain types over periods during which the towns or villages in question were rebuilt and remodelled several times. And inscribed or otherwise datable objects from the several layers prove that such structural periods do in fact denote substantial intervals of time. The range of Minyan ware over three or more centuries which seems to worry Dr Åberg is thus established by its association in various layers at the town of Phylakopi on Melos with well-dated Cretan imports ranging from rather before 1800 to nearly 1400 B.C.

On the other hand this same Minyan ware, so well fixed in the second millennium, is shown by the stratigraphy of a whole series of sites on the Greek Mainland from Asine and Tiryns to Eutresis and Orchomenos to succeed what are termed Early Helladic wares, which are in turn given a firm footing in the third millennium by association

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with Early Minoan imports from Crete both at Asine and Zygouries.² Now on the peninsula of Levkas in West Greece, Dörpfeld has excavated three cemeteries, one, the 'R graves', containing pure Early Helladic pottery, the rest, termed F and S cemeteries, Minyan but no Early Helladic vases. Yet Åberg speaks of the Levkas tombs as forming a 'more or less synchronous constellation' with the Mycenaean Shaft Graves (which contain Minyan), some others at Sesklo in Thessaly and Troy II. A 'synchronism' which thus cuts right across the stratigraphical division between Early and Middle Helladic is not very convincing. If a copper dagger with its gold mount from one of the R graves on Levkas approximates fairly closely to a rapier from the Shaft Graves dated by Åberg 1550-1450 B.C., it is no less like one from Mallia³ in Crete securely dated five hundred years earlier.

Even in barbarian Hungary we have stratified sites composed of a deposit 12 to 15 feet deep. The Nagy Sanc near Pecka (not far from Arad) is a *tell* 4 metres high, formed of fifteen distinct layers.⁴ Yet practically the whole of this deposit is in effect (p. 53) assigned to the first period of the Bronze Age (estimated at 250 years on p. 160) because the pins associated in a grave at another site with a vase characteristic of the 13th stratum are still of 'Aunjetitz type'. In point of fact, equally archaic types are quite often found in Hungarian graves of the Pannonian group which our author assigns to period II. It is surely less difficult to suppose that in Hungary fashions in pins persisted for say five centuries than that Pecka was remodelled thirteen times in half that space.

Absolute dates for the prehistoric periods among illiterate barbarians depend upon contacts between them and the historic civilizations. These are expressed to the archaeologist in three main forms: (1) actual importations of historically datable objects found in chronologically significant contexts; (2) obvious imitations of such made exceptionally among the barbarians themselves; (3) datable types adopted and manufactured locally among the illiterate peoples. Only in the first instance is a synchronism reasonably well established, but even so it is only certain if it be bilateral—if barbarian imports coming

² Seal impressions of Early Minoan III character from an E.H. III deposit at Asine (Persson, *Ärsberättelse*, Lund, 1923-4, p. 162; leg amulet of like age from E.H. tomb at Zygouries, Blegen, *Zygouries*).

³ Evans, *Palace of Minos*, II, 272.

⁴ Childe, *Danube in Prehistory*, p. 260, cf. p. 282.

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from the appropriate chronological horizon turn up in the historic civilization. The synchronisms between Egypt and Crete, between Crete and the Cyclades, are reliable just because they are of this bilateral kind. In the third case, on the contrary, the historically datable types only provide a *terminus a quo* for the barbarians' own products.

Down to the Hallstatt period the contacts between Cisalpine Europe and the historical world of the Mediterranean and Hither Asia are principally of the third kind. And they come mainly through Troy, itself a town of apparently illiterate barbarians datable only by the methods already described. It is the outstanding merit of Dr Åberg's present work that he has recognized the inconclusive nature of the arguments hitherto adduced for the absolute chronology of Troy II. Accordingly, in his attempt to delimit the beginning of the European Bronze Age, he sets out from a re-examination of the Trojan chronology. He relies mainly on a comparison between the metal objects from the 'treasures'—hoards buried just before the sack of the second city—and those from the Shaft Graves of Mycenae. He concludes that they must all belong to very nearly the same chronological horizon, that Troy II fell about 1500 B.C.

It is needless to examine in detail the agreements Åberg's analysis reveals. Some are very striking: both sites have yielded pommels of rock-crystal of the same type, which does show a typological advance over comparable objects from Early Minoan ossuaries; others are less convincing: granulation is attested as early as 1800 B.C.⁵ (Middle Minoan I), to say nothing of the Royal Tombs of Ur at least a thousand years older than the Shaft Graves. What is important is the utility of the comparison itself. For the Mycenaean material to be valid as limits for the Trojan it must be assumed either that the techniques and models reached Troy via Mycenae, or that both centres were inspired from Crete. Åberg makes this assumption, albeit with an explicit reservation (p. 157) in favour of some possible but unknown Oriental source. Unfortunately the assumption is demonstrably false. In the early Sumerian graves of Kish, Ur and Fara⁶ we find a whole series of distinctive types familiar from the Trojan hoards—tanged daggers, lock-rings, boat-shaped earrings, roll-head pins, whipped

⁵ Middle Minoan I at Mallia, and Middle Kingdom Egypt at Dahshur cf. *Bulletin de Correspondence Hellénique*, 1930, p. 416.

⁶ Heinrich, *Fara*, 1931, pl. 40; the data from the remaining sites are described in Childe, *The Most Ancient East*.

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loops—that are missing in Crete. And at least some of these same types have recently turned up halfway along the Royal Road that led from the Euphrates to the Aegean at Alishar⁷ in Cappadocia. In a word Trojan metallurgy and jewellery are rooted directly in Sumer, not in Crete. And so their limits must be fixed by Babylonian not Minoan-Mycenaean chronology. And whether we accept Woolley's high estimate of 3500 B.C. for the earliest Sumerian graves or reduce the figure with Christian⁸ to about 2650 those limits lie well back in the first half of the third millennium.

Of course the Sumerian dates provide only an upper limit for the Troad, three months' journey away. But other discoveries may help us towards a more precise demarcation. Even from the unsatisfactory summaries available one can infer a general parallelism between Troy II and the phase of Alishar contemporary with the 'Cappadocian tablets', 2200–2000 B.C. (Was the transmission of a whole cluster of Sumerian types to Anatolia connected with the Assyrian commercial activity revealed by those letters, or with earlier expeditions of the kings of Agade of which much later documents contain hints?). On the other hand the agreements between the Trojan treasures and the furniture of the R graves on Levkas upon which Åberg insists are complete, and these Leucadian grave-goods agree no less precisely with those from well-dated Early Cycladic tombs and Early Helladic settlements.

And if these arguments suffice to re-establish the claim of Bronze Age Troy II to a footing in the third millennium, other fresh facts push back still further the beginnings of the Anatolian Copper Age, represented by Troy I, which Åberg's table places about 1700 B.C. Thus Thermi was twice rebuilt before the foundation of Troy II. And similar hints are given in the east by the immense accumulation of debris termed Alishar I and by the appearance of ceramic types, closely allied to the Anatolian, at Ur and Uruk in the fourth millennium.

The discoveries at Ur and Kish incidentally transfer back to the third millennium the limits for the beginning of the European Bronze Age; for most of the Aunjetitz types represented at Troy also appear in the early Sumerian graves. The one type unrepresented in either area, the 'ingot torque', may go back nearly as far. It is found

⁷ cf. ANTIQUITY: a review of Erich Schmidt's *Anatolia Through the Ages*, p. 237.

⁸ *Archiv für Orientforschung*, 1931, 100.

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in Syria, and Dussaud has recently shown⁹ that the attempt to pin it down to Middle Kingdom times on the strength of a hoard found at Byblos must likewise fail; for the hoard is probably older than had previously been thought. It does not, however, follow that we must return to the old inflated chronologies for barbarian Europe. Contacts between the historical East Mediterranean civilizations and the barbarians north of the Alps still exist and are not entirely confined to the vague limiting cases that we have called class III.

Thus in what I call the Danubian II period, Åberg's Jordansmühl-Lengyel phase, we find in Hungary and Moravia curious clay cubes with one or two central cavities and lateral string-holes that reproduce every detail of an Early Minoan, proto-dynastic Egyptian and Early Sumerian stone paint pot.¹⁰ They are in fact indubitably clay copies of the stone vases and, since the examples are few and are all confined to one relatively short phase, they may rank as class II contacts for chronological purposes. A date rather before 2500 would accordingly seem plausible for the beginning of Danubian II and would become almost certain if Frankfort¹¹ were right in tracing 'Danubian' elements in Thessaly just before the belated arrival of the Early Helladic culture there.

Dr Åberg agrees that Danubian II overlaps with the early Passage Grave epoch in Denmark. This renders possible a rough check on the above date by evidence from two other channels of intercourse between the east and the north. A bone pin of south Russian type from an early deposit in a passage grave on Lolland establishes a class I synchronism between the early Passage Grave period and the Copper Age of the Pontic steppe.¹² This will some day be more accurately dated, but even now a stone battle-axe which, though locally manufactured, belongs to a specialized Pontic Copper Age type from an Early Macedonian layer suggests, as a *terminus ante quem* for that period, about 2000 B.C. The Atlantic route brought to Denmark about the beginning of the Passage Grave period or just before a peculiar copper dagger of a type distinctive of the corbelled tombs of Los Millares and

⁹ SYRIA, XI, 246.

¹⁰ Childe, *Danube*, p. 77; cf. Evans, *Palace of Minos*, II, p. 45, fig. 12, and Heinrich, *Fara*, pl. 12.

¹¹ *Studies in Early Pottery of the Near East*, II, 33ff.

¹² The point is fully discussed in MAN, 1931, 135.

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Alcalá in the Iberian Peninsula.¹³ This establishes a class I synchronism between very early Danish passage graves and some phase of Los Millares. These tombs cannot be precisely dated, but much of the pottery from them is so like that from rather similar vaults in Early Minoan Crete¹⁴ as to suggest a close approximation in time.

None of the above contacts in themselves would yield a strict synchronism, but the convergence of three quite independent lines of argument seems significant. A rough check is rendered possible by the few surviving pointers as to the date of the Aunjetitz culture; these do favour an overlap between that period and the Shaft Grave epoch. The clay copy of a Late Minoan I metal cup found in an Aunjetitz cemetery in Central Germany¹⁵ would be conclusive did it come from a scientific excavation. As it is, that probability fits in well with other fragmentary clues—the segmented faience beads from, again questionably, Aunjetitz graves in Moravia and Poland and contemporary or rather later burials in Spain and England, on the one hand, on the other the amber and the halberd from the Shaft Graves themselves. If we may therefore infer that the Early Bronze Age or Aunjetitz period had begun by 1500 B.C., we have two poles a thousand years apart between which must lie the Danish Passage Grave epoch and the events comprised therein—the beaker episode and the beginning of the Aunjetitz period. I still feel that the most coherent pattern is obtained by putting the last event about 1800 and the beginning of the Passage Graves some four centuries earlier. The Editor of *ANTIQUITY* having given me the opportunity of putting this pattern on a large sheet of paper, it has seemed best here to sketch the sort of arguments by which it might be defended even at the risk of appearing too critical of another system that may well prove more coherent and logical than my own. This means no disparagement of Dr Åberg's colossal industry, vast erudition or penetrating insight and no attempt to understate the ambiguities and uncertainty of the system I today prefer.

¹³ Nordmann in *De forhistoriska Tider i Europa*, II, 120.

¹⁴ Many unpublished vases in Siret's private collection are almost identical with those figured in Xanthudides, *Vaulted Tombs of the Mesará*, pl. xxv, bottom row.

¹⁵ Evans, *Palace of Minos*, II, 175.

Notes and News

COMMUNAL BURIAL

The problems of early British and European origins are closely bound up with the burial-customs of the late Stone Age. To this period belong the megalithic burial-chambers formerly called dolmens, and all the long barrows, of earth and stone. In these, communal burial of many bodies was the rule ; but the precise form of ritual observed—necessarily a matter of inference—is still disputed. The object of the present note is to call attention to a little known article of Rolleston's and to the references there given (' On the People of the Long Barrow period ', *Journ. Anthr. Inst.* October 1875, v, 120-73). Rolleston summarized the theories then prevalent as consisting of three—the Successive Interments, the Ossuary, and the Human Sacrifice theories. He pointed out that the first two are not mutually incompatible, and he rejected the third. In support of the ossuary theory he quoted some interesting evidence which deserves to be rescued from obscurity and reconsidered. He cited a passage in Alfred's Orosius,¹ from which it appears that ' it was the custom of the Esthonians to keep the body of anyone who died one month, or even two months, or, in the case of kings, even half a year, before burning it '. The other is from a letter from G. Turberville to the poet Spenser.² Writing from Moscow he says :—

The bodies eke that die unburied lie they then
Laid up in coffins made of firre, as well the poorest men
As those of greater state. The cause is lightly found,
For that in winter time they cannot come to break the ground.

This seems a reasonable and practical explanation of the custom of Reservation of the Dead. It might well be continued, in accordance with the well known conservatism of ritual, long after the necessity departed, though perhaps it will not explain all such occurrences. It fits in admirably however with what is known or suspected of the origin of British long barrow makers ; for it has long been clear that some of them came ultimately from the East of Europe. The present

¹ Pauli's *Alfred the Great* [and] . . . *Alfred's Orosius*, trans. by B. Thorpe, p. 255. (Bohn's series).

² Hakluyt's *Voyages*, ed. 1809, I, 433, and ed. 1903 (MacLehose), III, 128.

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writer suspected this long ago, basing his opinion on the resemblance between a round-bottomed pot from Danilov (Yaroslav)³ and the British neolithic vessels of West Kennet and Peterborough type. We now know much more about East Baltic and Russian pottery than we did then, and the resemblances are seen to be even more striking than was at first suspected. The presence, for instance, of both maggot-patterns and impressed holes (*grubchen-keramik*) on both classes of ware can surely not be fortuitous. Now this is precisely the region of winter-frozen soil where Reservation of the Dead was practised both in the time of Alfred and of the poet Spenser. May not our neolithic ancestors have observed a similar ritual in their Eastern homeland and brought it with them to this country?

Certain features of the bones in long barrows cannot however be completely explained by any one of these three theories. Occasionally the bones found have been deliberately broken into small fragments while still 'green'. This could only have been done by the exercise of some force. In a long barrow at Wexcombe excavated by the writer and Professor Hooton of Harvard in 1914, a lower jaw bone (and other bones) was broken in a way that seems to rule out chance. The character of the fractures was such as must have occurred either during life or (much more probably) soon after death, while the bone still contained much organic matter. (Sir Arthur Keith agreed with this opinion at the time). These facts, which have been observed elsewhere, demand some explanation. The theory of Human Sacrifice, though perhaps supported by some slight evidence⁴, is inadequate in such cases; and the others are irrelevant. O.G.S.C.

THE NAME OF THE GIANT OF CERNE

Direct evidence of any traditional name for the Cerne Abbas Giant is confined to a statement by Stukeley in 1764, that 'the people there give the name of Helis' to the figure.* This rather curious name does not appear to have survived to the present day, but there is evidence suggesting that a very similar name was given to the Giant in the 13th century.

³ Report of the International Congress of Anthropology and Prehistoric Archaeology, 7th Session, Stockholm, 1874, p. 296, fig. 37.

⁴ O. G. S. Crawford, *Long Barrows of the Cotswolds* (Bellows, Gloucester, 1925), p. 75; compare also p. 14.

* Minute Books, Society of Antiquaries, IX, 233, quoted by Colley H. March in *Proc. Dorset N.H. & Arch. Soc.*, XXII, 106.

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Sir Flinders Petrie has drawn attention to the 13th century version of the legend of St. Augustine's visit to Cerne at the end of the 6th century given by Walter of Coventry. This version, while differing but little in its essentials from those given by other medieval hagiologists, interpolates an important clause after the initial mention that Cerne is 'in Dorsetensi pago', adding 'in quo pago olim colebatur deus Helith'.¹ This, as Sir Flinders has pointed out, 'may preserve the early medieval name of the Giant'.²

There seems then a slender thread of evidence to suggest that for 500 years the Giant had been traditionally associated with a name Helith or Helis. It is of course conceivable that Stukeley's name was not a true local one, but unless derived from Walter of Coventry it is difficult to suggest his source. Here again 'deus Helith' may not refer to the Giant, but the identification is almost inevitable.

Support for this conjectural name of the Giant would be given if any parallel could be found in myth, tradition or folklore to a club-bearing giant with a name like Helith, for the earlier form is naturally to be preferred. Fortunately a fairly good parallel can be detected, which goes to confirm the identification.

An anonymous article in the *Quarterly Review* for 1902, discussed at length by Mr R. Lowe Thompson in his recent *History of the Devil*, traces the Harlequin of pantomime, via the Arlechino of the Comedia del' Arte, to a group of medieval legends centreing round a demon figure, particularly a 'wild Huntsman' leading a troop of damned souls, who frequently appears in medieval French stories under some such name as Helequin or Hierlekin. The earliest (late 12th century) reference to 'familia Herlechini' describes him as a club-bearing giant. Now in the English forms of the legend the Huntsman is confused with his Hunt by popular etymology taking the 'kin' to indicate the followers, and thus creating a new leader, Herle. But more particularly to our purpose is an English form of Helethkin (probably due to the influence of OE 'heoloth-cynn', the hell-folk). This form does seem to provide the necessary link with Walter of Coventry and the Giant, and give reality to the existence of Helith as the medieval name of a spectral club-bearing giant.

¹ *Memoriale*, ed. by Stubbs. Rolls Series, 1872, I, 60.

² *The Hill Figures of England*, p. 10. Occasional paper. No. 7, Royal Anthropological Institute, 1926.

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The ultimate origin of this legendary figure seems probably to be found in some form of Hercules, and I am informed that although the early French forms of the name would be a quite irregular development, philological connexion between Herlechin and Hercules is not out of the question. The Italian Arlechino in his earliest days seems to have been a figure closely resembling Hercules in his attributes.

An additional note may be added on the repeated statement in the English editions of Camden that St. Augustine founded Cerne Abbey 'when hee had broken there in pieces Heil the Idol of the heathen English-Saxons, and chased away the fog of paganish superstition' (1637, p. 212). Heil suggests connexion with the Giant and with Helith, but the source of the statement has not been traced. Gibson gives William of Malmesbury as authority, but although this writer in his *De Gestis Pontificum Anglorum*³ gives a very full account of Augustine's visit to Cerne, no mention is made of the overthrowing of Heil. Nevertheless the missionaries met with a very hostile reception, the crowning insult of which appears to have been the fixing of fish tails to the saintly garments—'ita ut etiam caudas racharum vestibus ejus affigerent'. The incident is very curious, and its significance more than obscure, but the suggested reading 'caudas vaccarum'—cows' tails, is perhaps preferable to 'caudas racharum'—'tayles of thornback or lyke fysshes', as the Golden Legend version of the incident, which is however localized in Kent, gives it.

The miraculous and edifying termination of the legend as there given makes a fit conclusion. When the people had driven Augustine out 'he besought Almyghty God to shewe hys judgement on them; and God sent to them a shamefull token; for the chyldren that were born after in the place, had tayles, as it is sayd, tyll they had repented them. It is sayd comynly that this fyll at Strode in Kent; but blyssyd be Gode, at thys daye is no such deformyte'. Nor is there, I understand, at Cerne.

STUART PIGGOTT.

PALAEOLITHIC AXES FROM TRANSJORDAN

When I was in the East in 1928 I had the good fortune to visit several inaccessible sites in the company of Group-Captain Rees, V.C., then in command of the Royal Air Force, Transjordan. On one of

³ Rolls Series, 1870, 184.

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the basalt-capped hills,* on the level surface at the top, I picked up one of the fine specimens here illustrated (fig. 1); and as we were descending the hill Group-Captain Rees picked up another (fig. 2). Both have a brown leathery patina, and are evidently of great age; they contrast strongly with other worked flints to be found on the top of the hill, which are much lighter in colour, and quite devoid of the typical brown leathery patina. This patina closely resembles that so familiar on Egyptian specimens, such as may be picked up in the desert near the Pyramids of Giza.

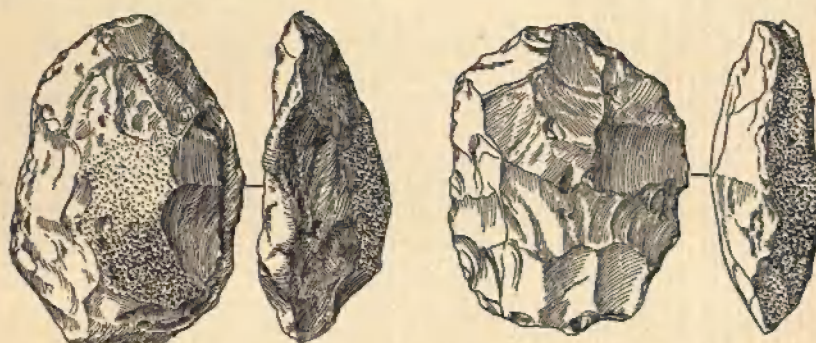


FIG. 1.

FIG. 2.

PALAEO-LITHIC AXES FROM TRANSJORDAN

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The hill itself is residual, the remnant of a limestone plateau covered with a thin sheet of basalt. At some date subsequent to the deposition of the basalt, this plateau has been dissected by the action of rain-water; this does not however necessarily indicate a change of climate, for rain does fall still over the area, and pretty heavily at times.

Both in type and, to some extent, in patina these Transjordan hand-axes resemble those from the high ground round Basingstoke, now to be seen in the Basingstoke Museum.† Amongst these is a tortoise-core. It is not unlikely that the artifacts from these two very widely separated regions may be contemporary, and that their patina may be the result of exposure to sun and rain throughout millennia. Similar causes produce similar effects.

O.G.S.C.

* Lat. $31^{\circ} 48' 30''$ N; Long. $37^{\circ} 30' 30''$ E. The axes are now in the British Museum.

† See 'The Antiquity of Man in Hampshire', by J. R. Ellaway and G. W. Willis, *Proc. Hants. Field Club*, 1927, IX, 178-188.

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HARPOON FOUND IN THE NORTH SEA

Dr Muir Evans has presented to the Castle Museum, Norwich (to whom we are indebted for the photograph opposite) a bone harpoon of the Kunda type. It was discovered in a lump of peat (known by the seamen as 'moor-log') dredged up by the trawler *Colinda* from a shoal called the Leman and Ower sands 50 miles northeast of Cromer (about lat. $53^{\circ} 10'$, long. 2° east). As will be seen from the illustration, it closely resembles the Holderness specimens, whose authenticity has been disputed (though never by us); he would be a bold man who would dispute the authenticity of the present specimen.

It is of great scientific value, for it proves—what indeed was obvious already—that during the Maglemose period, there was fresh water at a spot now many feet below the sea. The presence of man in Britain at that time is already fully vouched for; but how did he get here? The answer to the question is that he walked across either what is now the North Sea or the English Channel.

We know little about the mesolithic period in Britain, and it is good news therefore that a book on it, by Mr J. G. D. Clark, will shortly be published by the Cambridge University Press.

ARCHAEOLOGICAL RESEARCH IN TURKEY

We have received the following interesting report* of German archaeological research in Turkey, 1924–31, from M. Schede, Director of the Deutsches Archäologisches Institut, Constantinople.

German archaeologists resumed their research-work in Asia Minor and Constantinople in 1924, and, as before the war, they received every kindness from the Turkish government. Their first object was to complete the excavation of the temple of Apollo at Didyma, begun by Wiegand and Knackfuss in 1908, and in this were successful, after clearing away the rubble on the north and west sides. This temple, the ancient oracular shrine of Miletus, is now the most important ruin in Asia Minor, and the best-preserved and most instructive example of Hellenistic architecture. Nothing unexpected was found, for the earlier excavations had made the site perfectly plain as regards its plan and lay-out, and the investigation was finished in 1925.

Excavations were also made in 1924 in the Troad on the bay of Besika under the direction of O. Mey, assisted by W. Dörpfeld and

* Translated by Roland G. Austin.



HARPOON DREDGED FROM THE LEMAN AND OWER
SANDS, 50 MILES NORTHEAST OF CROMER

(about lat. $53^{\circ} 10'$, long. 2° east).

Now in the Norwich Castle Museum. Exact length $8\frac{1}{2}$ inches

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M. Schede.¹ The main purpose was to ascertain whether the anchorage of the Greeks was here and not in the Dardanelles, and whether the Ujektepe was the common tomb of Achilles and Patroclus while the Besika-tepe was that of the Achaeans. The first problem could not be solved, because the ground-water prevented deep excavations; the work done on the Ujektepe confirmed Schliemann's view that it was a site belonging to the archaic period of Greece and occupied again in Roman times, while the excavation of the Besika-tepe had to be prematurely abandoned.

More important investigations were renewed at Pergamum by T. Wiegand, with the help of G. Oberländer, and the work has been continued each year since 1927. This is the third period of excavation of the site since it was first opened up by Humann and Conze in 1878-86 (when the exhibits in the Berlin Museum were discovered). Between 1900 and 1914, the work was continued by Conze and Dörpfeld. So far Wiegand has examined three sites²: (a) the so-called Palace of the Princes, near the great altar, which proved to be a complicated building of the royal period, of uncertain purpose; (b) the 'Garden of the Queen', on the top of the citadel, where arsenals and a barracks of the early Hellenistic period were discovered; (c) the shrine of Asclepius outside the city, where the physician Galen worked, which was found to be an ambitious lay-out dating entirely from the second century A.D. Two very interesting circular buildings were discovered, one being the temple itself, adjacent to which was a large festival-place, bounded on the south by a two-storeyed hall 126 metres in length and connected at its northwest corner with a great theatre (the fourth to be found at Pergamum). The water-course of the ancient healing spring, which was led to the rotunda below through a vaulted conduit 80 metres long was found, and also some important inscriptions, and some good sculptures, one of which is a heroic statue of Hadrian in the nude. These excavations should be completed this year.

Work at Ephesus for the Turkish State Museum in Smyrna and the Austrian Archaeological Institute, was begun in 1926 under the direction of Josef Keil.³ This was made possible through the

¹ Mey, *Das Schlachtfeld von Troja*; Dörpfeld, *Studien zur vorgeschichtlichen Archäologie*, Alfred Götzte dargebracht 115 ff.; Schede, *Arch. Anzeiger*, 1929, pp. 358-65.

² Wiegand, *Bericht über die Ausgrabungen in Pergamon*, 1927 (*Abhandl. Berl. Akad. d. Wissensch.* 1928); Wiegand, *Arch. Anz.*, 1927, 466 ff.; 1930, 191 ff.; 1931, 308.

³ Keil, 'Bericht über die Ausgrabungen von Ephesos', *Jahreshefte des Österreichischen Arch. Instituts*, bd. 23-27, XII-XVI, and 'Führer durch Ephesos', 1930, *Arch. Anz.*, 1930, 444 ff.

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generosity of Mr Rockefeller, and by grants from the Turkish Government, the Forschungsgemeinschaft der deutschen Wissenschaft, and the Austrian Ministry of Education. The results of the excavations included the discovery of a metroon, with many votive reliefs dedicated to Cybele ; a temple dedicated to Domitian, and the so-called temple of Claudius, which was probably a shrine of Serapis. Three of the city's gymnasia were laid bare, the one at the Stadium yielding many fine sculptures, chiefly copies of famous classical works of art : among them a relief of Icarius, the Hestia Giustiniani, and the Discobolus. A number of late-period (*spätantike*) sarcophagi were found in the Necropolis.

The examination of Christian shrines gave valuable results. The grotto of the Seven Sleepers, one of the most celebrated places of pilgrimage of the Middle Ages, proved to be a great and ambitiously planned coemeterium of several storeys, with hundreds of grave-sites. The church of St. John, built by Justinian on the site of the evangelist's grave, was completely uncovered, thus adding to our knowledge of the oldest mausoleum of the district.

In 1926 and 1928 D. Krencker and M. Schede, acting for the Deutsches Archäologisches Institut and the Forschungsgemeinschaft der Deutschen Wissenschaft, gave their attention to the Roman temples of Ancyra and Aezani,⁴ which are in a comparatively good state of preservation, and should be studied in every detail in order to increase our knowledge of the history of art in Asia Minor under the Empire. French archaeologists have already done good work at both sites : Guillaume at Ancyra⁵ and Landron at Aezani⁶, but considerable advances were made on their discoveries. At Ancyra it was possible to excavate at various points, and the problem of its pillar-structure was solved. It stood on a podium, and was pseudodipterous. The pronaos had four pillars in front of the wall-ends (*antae*), the opisthodomus two pillars between them. The order was probably Ionic. The temple of Augustus was thus remarkably like the temple at Aezani, though about 100 years older, for here too the position of the four-pillared prothesis

⁴ Krencker, *Gesellschaft von Freunden d. Techn. Hochschule Berlin zu Charlottenburg*, 1927. Schede, *Gnomon* 2, 1926, 746 f. ; 5, 1929, 60 f.

⁵ Perrot, Guillaume, Delbet, *Exploration archéologique de la Galatie et de la Bithynie*, 1862.

⁶ Lebas, *Voyage archéologique en Grèce et en Asie mineure*.

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was found to be in front of the pronaos, contrary to the views of Lebas and Landron. At Aezani the structure is so well preserved that only slight excavations were needed to give a clear picture of nearly all the temple buildings, and it has now become one of the best known of ancient architectural sites. The most striking parts are the front architraves on which was the inscription of consecration, although admittedly only the holes for the insertion of the bronze letters are left, and also the middle pediment-angles (*acroteria*), where imposing busts of divinities were found in the scrolls. The altar stood in front of the temple, and beyond was a beautiful propylaeon, through which access was gained to the halls of the large temple court, with their fine decorations of the Antonine period. Near Aezani lies the cavern of Meter Steunene (Anderson, *British School Annual*, 4, and Wiegand, *Athen. Mitt.*, 1911), and here were found many fragments of Hellenistic pottery, which proves that the shrine itself was older than the lay-out of the temple.

In October 1929 the recently-founded Istanbul centre of the Deutsches Archäologisches Institut began work under the direction of P. Wittek, acting for Turkey, and M. Schede for Germany. Hitherto German archaeologists have been represented here by a member of the Berlin Museum. One of the objects of the Institute is to survey important monuments still *in situ*, and the obelisk of Tutmosis III in Constantinople was thoroughly examined by G. Bruns, and the aqueduct of Valens by K. O. Dalmar, while K. Stein did similar work on the city walls of Nicaea.⁷ A survey of the walls of Constantinople will be carried out by H. Lietzmann, F. Krischen and others, acting for the Forschungsgemeinschaft der Deutschen Wissenschaft.⁸

In the autumn of 1931 the Deutsches Archäologisches Institut, together with the Deutsche Orientgesellschaft, undertook excavations at Boghaz Keui, the ancient Hittite capital, in which thousands of cuneiform inscriptions and striking architectural remains were found in 1907 by Turkish and German archaeologists. Work was begun at the citadel of Büyük-kale, under the direction of R. Bittel; the stratification of the ceramic was accurately noted and related to certain buildings. A number of cuneiform tablets were again found: not however in their original positions but used as rubble. The work here is to be continued, and promises valuable results.

⁷ *Arch. Anzeiger*, 1930, 462 f.

⁸ Lietzmann, 'Die Landmauer von Konstantinopel', *Abh. der Berl. Akad.*, 1929; *Arch. Anz.*, 1929, 339.

Recent Events

The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.

On a rock near a dolmen in the department of Charente Inférieure has been found a sculptured hand with some of the fingers missing. (*Bull. Soc. préh. franç.*, January 1932, XXIX, 62).



With reference to the 'coin-in-the-slot' machine referred to on p. 111 of our last number, Sir George Macdonald writes :—' Your contributor will find a description of it in Heron of Alexandria, Πνευματικά, I, 21. The passage is reproduced, along with an excellent diagram, in Wilamowitz-Moellendorf's *Griechisches Lesebuch*, 2^{te} hbd, p. 261. The principle is, I believe, identical with that of modern "penny-in-the-slot" contrivances '.



Slum-clearance in Exeter has led to an effort to recover the plan of Roman Isca. Preliminary work has been done, and to continue it on a larger scale money only is required. Contributions may be sent to the Treasurer, Rear-Admiral F. B. O'Dogherty, Old Fairfield, Chudleigh, Devon.



We learn, through the good offices of one of our readers, that the ancient harbours of Carthage are in a sorry state. Situated in the middle of the modern village of Salambo, they no longer function as in ancient times ; indeed their size is so small that one wonders how they can ever have held the multitudinous craft that frequented them. Now they have become no more than a cess-pool for the adjacent houses ; and when we are told that in summer they become nearly dry, we can understand that their annihilation is demanded by residents. Two courses are open—to fill them up or to restore their connexion with the sea ; and we hope that, in spite of the expense, the latter course may be adopted. Their historic interest is great ; nor is it unlikely that,

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in the process of clearance, valuable archaeological discoveries will be made. Surely there must be at the bottom of them a host of valuable objects lost overboard in ancient times? The Tunis Museum has already been enriched by submarine discoveries (see *ANTIQUITY*, 1930, iv, 405, 408) and here is a chance of yet further riches.



Great success has attended the excavation of Mintarno in Central Italy, on the Gulf of Gaeta. Let us hope that it will be crowned by a speedy and adequate publication of results. Amongst the buildings revealed are the following :—Remains of an Augustan period temple ; an Italiote temple of the period of Sulla with a great and mostly intact sanctuary wall ; a second century A.D. water-clock or fountain ; the Appian Way in its unbroken and original progress through the town, with cross streets, man-holes, and sewers all perfectly intact ; a stretch of fourth century B.C. fortification wall of stone in opus quadratum ; an adjoining still earlier wall of polygonal construction, and at the point of junction of these walls a massive tower of the Augustan age ; two buildings in the temple area of fourth and third century B.C. construction ; another part of the city wall going back to the fifth century B.C. ; a colonnade of the fourth century B.C. ; three temples of the third and first centuries B.C. and the first century A.D. ; an elaborate nymphaeum of the time of the Emperor Hadrian ; and the arcade, proscenium, and orchestra pit of the theatre. (*Glasgow Herald*, 25 January).



A copper celt (axe) has been found at Ipplepen, Devon, in digging the foundations for some new houses. (*Devon & Exeter Gazette*, 1 February 1932). A similar find came to light recently on Hazel Down near Longstock Park, Hants. (*Antiq. Journ.*, xii, 70, 71).



A branch of the Edinburgh League of Prehistorians has been formed at St. Andrews. In the course of an address at the inaugural meeting Professor Gordon Childe referred to 'Gaulish walled forts near Abernethy and Forgandenny [which] were proofs of a hitherto unsuspected landing of Celts about 250 B.C., who must have sailed direct from Gaul without passing through England'. (*Glasgow Herald*, 6 February).

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In our last number we referred (p. 95), with all reserve, to the alleged authentication of the Pleistocene age of the Oldoway skeleton. We did not conceal our doubts, and they are evidently shared by Professors D. M. S. Watson, of London University, and C. Forster Cooper, of Cambridge University. In a letter to NATURE (27 February) they adduce reasons in support of their ' conclusion that the Oldoway man reached the position in which he was found by an artificial and probably a relatively recent burial ' ; and they challenge the original discoverer and his associates to explain the grounds on which they based their opinion.



The importance of the alleged great age of the skeleton lay of course in the fact that it is in every way a specimen of modern man (*Homo Sapiens*) ; and it would be by far the oldest representative if the claim were substantiated. As it is, it looks as if Oldoway man would go the way of his predecessors of Galley Hill and Ipswich—the way that leads from the main hall to the cellar of the museum.



Dr L. H. DUDLEY BUXTON writes :—' Professor Dreyer of Grey University, Bloemfontein, South Africa, has reported in a South African paper an ancient human skull of uncertain geological age but in a complete state of fossilization. The short face, well developed chin and very large mandible are in Professor Dreyer's opinion in no way primitive. The cranial features are however extremely abnormal, being rather within the range of microcephalic idiots than of normal human beings. The estimated cranial capacity is below 900 cc. It is probable therefore that we are faced either with a pathological specimen or else with a hitherto unknown type of man. It is hardly possible from a brief description and a bare outline to form any definite opinion one way or the other, and a full description of the specimen will be awaited with great interest. It is not necessary to remind readers of ANTIQUITY of the great importance of finds made in South Africa in the last few years and it is most satisfactory to know that Professor Dreyer, who has described the skull mentioned above, has recently made extensive finds of human remains in the Matjes river shelter associated with a variety of cultures from Mossel Bay through a not quite typical Smithfield to examples of the Wilton type '. A paper on these skulls was read at the Royal Anthropological Institute by Sir Arthur Keith on 8 January last (*The Times*, 9 January 1932).

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The band-keramik village outside Cologne is yielding most interesting plan-results. A narrow trench has been discovered dividing the enclosed area into two unequal portions. Future work will, it is hoped, determine whether the smaller of these was for cattle ; since a pond has already been found in it, this seems highly probable. The domino-like arrangements of, usually, nine post-holes are explained as the supports of granaries. It was necessary to raise wheat above the ground to preserve it from decay, and, we may add, mice and rats. Similar raised granaries were found by General Pitt-Rivers in the Romano-British village of Rotherley. (*Excavations in Cranborne Chase*, II, 55).



We wish the excavators the best of luck, and hope that this exceedingly well conducted excavation will be carried to a successful conclusion. A complete plan of one village will be a great achievement. *Finis coronat opus*. (Dr Werner Buttler in *Forschungen und Fortschritte*, 20 February 1932, 65-67 ; see also *ANTIQUITY*, 1931, V, 227-9).



On page 498 of our December number (no. 20) we referred to Miss D. A. E. Garrod's excavations ; these were carried out by her on behalf of the British School of Archaeology jointly with the American School of Prehistoric Research (C. G. MacCurdy, Director). We regret that, in our original reference, we omitted to mention the name of the American School of Prehistoric Research. The omission was, it need hardly be said, purely inadvertent. On account of the ramifications of research organization it is not always easy to avoid errors of this kind when one is reporting archaeological news, but we sincerely regret the oversight.



The excavations at Salmonsbury, near Bourton-on-the-Water, Glos., will be resumed this summer if sufficient financial support is forthcoming. We cordially recommend these excavations to our readers as worthy of their support ; a preliminary report on the first season's work will be found in *ANTIQUITY* (December 1931), V, 489-91. Contributions should be sent to Mr E. W. Kendall, Hon. Treasurer, Bourton-on-the-Water, Glos.



The ninth annual interim meeting of the Anglo-American Historical Conference will be held at the Institute of Historical

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Research, Malet Street, W.C.1., on Friday, 1 July 1932. The Continuation Committee will present their report at 2.30 p.m., after which Professor J. L. Myres, of the University of Oxford, and Professors H. J. Fleure and E. F. Jacob, of the University of Manchester, will open a discussion on 'The Relations between Historians, and Geographers and Archaeologists'. There will be an informal gathering at the Institute at 8 p.m. Further information may be obtained from the Secretary of the Institute.



Excavations have been continued at Nineveh under the direction of Dr R. Campbell Thompson, who reports to *The Times*, 11 April (p. 8) on the results. The most important work was digging a pit about 90 feet deep to virgin soil through five strata. The usual polychrome pottery (dated by the finder at about 3300 B.C.) was found in the uppermost stratum; the second contained fragments of polychrome ware claimed to be many centuries older.



The Viennese Academy of Sciences and the Egyptian Museum of Stockholm have completed the excavation of the neolithic settlement at Merimde, near Beni-salame, about 32 miles northwest of Cairo, the results disclosing much which concerns the life, customs, and crafts of the ancient inhabitants. (*The Times*, 15 March, p. 13).



As the result of discoveries in the Wady Ghazze (Gaza) Sir Flinders Petrie reports some interesting customs associated with the introduction of the horse into Egypt and Palestine. (*The Times*, 25 February, p. 10).



A useful summary of important objects of antiquity added to the Italian Museums during the 10 years of the Fascist régime is published in *The Times*, 8 April, p. 12. They have been arranged, with other exhibits, in the National Gallery of Modern Art in Rome.



An expedition from Yale University under the leadership of Professor Hellmut de Terra, research associate in geology, is to explore the Himalaya and Western Tibet for evidence of the theory that Man originated in Central Asia. (*The Times*, 19 April, p. 17).

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The discovery of a 'Celtic' farm-pond near the top of Nore Hill, Eastham, Sussex, is described in some detail by Mr S. E. Winbolt. (*The Times*, 19 April, p. 19).



A report on recent additions to the Cairo Museum is printed in *The Times*, 19 April, p. 13, the most important being the Tutankhamen finds. There are also some funerary remains from a cemetery at Kostol, on the east bank of the Nile, where the Egyptian Government Survey, under the direction of Mr Emery and Mr Kirwan, have done some useful work in the huge tumuli of early Byzantine times.



The Rome correspondent of *The Times* has received a report from Dr Luigi Ugolini, leader of the Italian Archaeological Mission in Italy, on his work at Butrinto, Albania, where attention during the past season has been given to the completion of the excavation of the theatre. The auditorium and the orchestra date to the fourth century B.C. The stage, which is in excellent preservation, is of Roman construction and rests in part on the remains of the Greek original. Many inscriptions, and some good statuary have been found. (*The Times*, 19 April, p. 13).



The discoveries on the site of the Roman amphitheatre at Chester were described recently by Professor Newstead in a broadcast talk, the substance of which is given in *The Times*, 18 April, p. 9. The date of the structure as shown by the pottery is placed towards the end of the first century A.D. We cordially support the appeal which is being made for funds to preserve it.



Dr H. Frankfort, Director in Iraq of the Oriental Institute, University of Chicago, writes to *The Times* (26 March, p. 15), on new evidence of intercourse between India and Babylonia shown by a seal found at Tell Asmar, the site of ancient Eshnunna, about 50 miles northeast of Baghdad. Dr Frankfort states that the seal is without parallel among known cylinders. Certain peculiarities connect the seal definitely with the Indus civilization and there is no doubt it is an importation from the Indus Valley which reached Eshnunna about 2500 B.C.

Some Recent Articles

This list is not exhaustive but may be found convenient as a record of papers on subjects which are within the scope of ANTIQUITY. Books are occasionally included.

Some recent contributions to the Pleistocene succession in England, by K. S. Sandford. *Geological Magazine*, Jan. 1932, LXIX, 1-18.

A brief critical summary of recent work, with attempted correlations between the Upper Thames, the Lower Thames, and East Anglia; together with comments on the need of coordination of research concerned with these problems. No student of human origins can afford to miss this paper. We have in England a wealth of evidence bearing closely on man's antiquity, but it needs disentangling, and here we are given a general view of the present state of the process.

Climate, vegetation and man in the Huddersfield district, by T. W. Woodhead. Tolson Memorial Museum Publications, Handbook VIII, 1931. Paper 1s; limp cloth 1s 3d.

One of the completest regional studies of its kind in existence, fully illustrated by photographs of models (relief-maps); and concluding with a short chronological table from 1100 B.C. to A.D. 1000.

Post-glacial succession of forests in Europe, by T. W. Woodhead. *Science Progress*, October 1931, no. 102, 250-61.

Another attempt at correlation, with a table particularly detailed in regard to vegetation.

Zur Megalithkultur Nordwest deutschlands, by Dr Ernst Sprockhoff (Römisch-Germanisch Zentral-museum, Mainz). *Nachrichten aus Niedersachsens Urgeschichte*, no. 4, 1930. (August Lax, Verlags-handlung, Hildesheim). pp. 56, 23 illus.

A very useful account with many plans and three distribution sketch-maps.

Notes de préhistoire palestinienne, by R. Neuville (Jerusalem). *Journ. of the Palestine Oriental Society* (Jerusalem), 1930, x, 64-75; 193-221.

Valuable notes on the classification and relative age of flint implements in Palestine—a subject which has been neglected by most of the excavators in that country. Full cognizance is taken of Miss Garrod's work. No plans or sections, however. (A criticism of the author's dating is made in the article by Dr Albright quoted in the next notice).

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Recent progress in the late prehistory of Palestine, by W. F. Albright. *Bull. Amer. Sch. of Oriental Research* [BASOR], April 1931, no. 42, 13-15.

The following paragraph is worth quoting in full:—'We have now, as a result of less than three years of work on the part of a small group of scholars, a nearly complete classification of the more important successive cultures of Palestine at the end of the Stone Age. First there comes Natufian, parallel (roughly speaking) with the Tardenoisian of Europe and the latest Capsian of Egypt. Agriculture and presumably the domestication of animals begin, but pottery is unknown. Next we have the age of the first potters, the age when copper began to be used, the period of the Campignian of Europe and Faiyumian and Badarian, etc., of Egypt. Mesolithic now becomes Neolithic, not later than 4000 B.C., at the lowest computation. Nothing so far found in Palestine appears to belong in this age, which is thus a lacuna to be filled by the Palestinian archaeologist. Then comes the characteristic Chalcolithic of Palestine, the Ghassulian, which has been found in the caves of Galilee and Carmel as well as in the lower Jordan valley. Finally after 3000 B.C. we have the Early Bronze, with its Tahonian flint industry'.

The Excavations at Nuzi: preliminary report of the fourth campaign, by Prof. Robert H. Pfeiffer. BASOR, April 1931, no. 42, 1-7. (Air-photograph, giving plan of excavated ruins).

On the map found at Nuzi, by W. F. Albright. BASOR, no. 42, 7-10, illus.

We hope to revert later to this very interesting clay-tablet of the 3rd-millennium.

Letter of February 28 [1931] from Dr Speiser to the President of Dropsie College and the Director of the American School at Baghdad (Second campaign at Tepe Gawra). BASOR, April 1931, no. 42, 10-12.

Describes work at this very important early tell, near Mosul. 'The peculiar fascination of Tepe Gawra is due to the fact that, of its twenty-odd strata, only four are later than the third millennium; more than ten are of the aeneolithic or prehistoric period, which belongs primarily to the fourth millennium. These numerous prehistoric levels make the mound the oldest in Iraq, a fact that can scarcely be overstated'. A critical study of the flint (and obsidian) implements, especially those from the lowest levels, seems greatly needed; no doubt such will be published in the full report. A 'potful of charred wheat' was found in a layer assigned to the fourth millennium ('Gawra 1').

Excavations in the Wady el Mughara, 1931, by Dorothy Garrod. *Palestine Exploration Fund, Quarterly Statement*, January 1932, 46-51.

Records the progress of excavations during 1931 in the Wady el Mughara by Miss Garrod and her colleagues. Deposits of Mousterian and Natufian (mesolithic) age were found, together with 17 Natufian burials and the skeleton of a Mousterian child (2½ years old) of 'definite Neanderthal type' (Keith). The lower Natufian contained sickle-blade hafts.

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Opportunism and the factors of invention, by H. S. Harrison. *The American Anthropologist*, Jan.-March 1930, XXXII, no. 1, 106-25.

An important paper, by the Director of the Horniman Museum, on the fascinating subject of tools, treated (we think we may say) from an 'organic evolutionary' point of view. It is not easy reading, but it must be studied by sociologists as well as by anthropologists, as it deals with fundamental matters.

Die Alamannen in Württemberg, by Walther Veeck. 1931. [Germanische Denkmäler der Völker-wanderung-zeit. Issued by the German Archaeological Institute, Frankfurt-on-Main]. Walter de Gruyter, Berlin W. 10. Paper, RM 65; bound, RM 75.

An aerial expedition to Central America, by Percy C. Madeira, Jr. *Museum Journal* [Univ. of Pennsylvania, Philadelphia], June 1931, XXII, 95-147.

The Chambered Cairn of Bryn Celli Ddu, by W. J. Hemp. *Arch. Camb.*, Dec. 1931, LXXXVI, 216-62.

In part a summary of and in part supplementary to the article in *Archaeologia*, LXXX, 179-214, by the same author.

The Administration of Archaeology in Wales in 1931. Presidential Address by R. E. M. Wheeler. *Arch. Camb.*, Dec. 1931, LXXXVI, 340-52.

Roman Britain in 1930, by R. G. Collingwood and M. V. Taylor. *Journal of Roman Studies*, 1931, XXI, 215-50.

The usual, and indispensable, record of excavations, inscriptions and other discoveries.

Notes on some Romano-British pigs of lead, by G. Clement Whittick. *Journal of Roman Studies*, 1931, XXI, 256-64.

Saxon Lydd, by Gordon Ward. *Archaeologia Cantiana*, 1931, XLIII, 29-37.

An interesting topographical study—Saxon bounds, field-work.

Notes on some West Kent roads in early maps and road-books, by E. G. Box. *Arch. Cantiana*, 1931, XLIII, 85-98.

The Sturge Collection: an illustrated selection of flints from Britain bequeathed in 1919 by William Allen Sturge. By Reginald Smith. London: British Museum, 1931, 25s.

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Notes on (1) certain prehistoric relics from Orkney, and (2) Skara Brae : its culture and its period, by J. Graham Callander. *Proc. Soc. Ant. Scotland*, 1930-31, LXV, 78-114.

The early Bronze Age flint dagger in England and Wales, by W. F. Grimes. *Proc. Preh. Soc. East Anglia*, 1931, VI, 340-55.

Notes on the Beaker pottery of the Ipswich Museum, by J. G. D. Clark. *Ib. id.* 356-61.

The conclusions reached by the writer of this and the preceding paper are in agreement, though reached quite independently.

Archaeology of the Arkansas River Valley. By Warren King Moorehead. (With supplementary papers). pp. 205. Illustrations and sketch maps. *Yale University Press : London, Humphrey Milford.* Price not stated.

The illustrations depict a quantity of beautiful pottery finds in the excavations, and also figure in assiduous detail the stone implements, both chipped and polished, which were here as characteristically commingled as in many another American area. . . . Mr J. B. Thoburn writes on the prehistoric cultures of Oklahoma and makes interesting, if not always pertinent, observations on the lives and activities of the Caddoan peoples, on the possible migrations and counter-migrations which originated the so-called Canadian Culture . . . Mr F. V. Studer devotes several pages to an account of his observations in the Texas Panhandle ruins, and reproduces pictures from the wall of a rock-shelter near Folsom, New Mexico, which might well have been executed in late Palaeolithic France or Spain.

J. LESLIE MITCHELL.

Archaeological Atlas of Michigan. By Walter B. Hinsdale. *The Librarian, General Library, University of Michigan.* pp. 38 and 20 maps. 6 dollars.

A beautiful and almost faultless production. The preliminary pages give a brief sketch of the Michigan cultures, and a brief description of the different types of sites figured in the maps. The maps have been executed with a commendable elimination of unnecessary detail, and are as full as any archaeologist could wish.

J.L.M.

British Columbian Ancestors of the Eskimo ? By Professor Charles Hill-Tout. *Illustrated London News*, 16 January 1932.

An account of excavation in British Columbian kitchen-middens. A trephined skull, some wampum necklaces, a figurine and various stone ceremonial bowls are illustrated and described. The facts seem hardly to warrant either the headline or the hypothesis, and the 2500 years assigned to the deposits might well be cut in

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half. The dolichocephalic pre-Salish autochthones appear to have absorbed considerable elements from an intrusive culture—or were themselves intrusive Californians, rather than ancestors of a people so remote in space and time as the Eskimo. J.L.M.

Fire Worship in Britain. By T. F. G. Dexter. New Knowledge Series (no. 4). *Watts & Co.*, 1931. pp. 47. 1s.

It is unfortunate that Dr Dexter's very readable little work should appear under such unpropitious auspices. The verbiage of the prefatory 'Synopsis of Series' savours less of 'new knowledge' than of certain theorizings, unacceptable to most working anthropologists, and decidedly stale. A certain flavour of 'Easterners' (together with irrelevant illustrations) is, moreover, detectable in the text. The attitude of the early Church to Pagan festivals and deities, however, is well drawn.

It was indeed 'the policy of conciliation and incorporation pursued by the early Church' that conserved the material of Dr Dexter's study. Among the illustrations are several amusing reproductions from old prints of May Day, Midsummer and Twelfth Night festivities. J.G.D.C.

Archivo de prehistoria levantina (Anuario del servicio de investigacion prehistorica de la excma. Diputacion provincial de Valencia). Vol. I, 1928. pp. 262, with 54 plates and numerous illustrations in the text. Price not stated.

This first volume is a handsome production that does credit to the 'Servicio de investigacion prehistorica' of Valencia, and is in every way worthy of the excellent series of papers it contains; indeed, if this new journal appears with reasonable punctuality and maintains its present high standard in the matter of printing and illustrations, then the 'machinery' of Spanish archaeology will be very considerably improved, and we shall all be correspondingly grateful to the enterprise of the Director, Sr. Ballester, and his colleagues. In this volume the most important paper is the description of the Iberian town 'La Bastida', which is illustrated by a plan and no less than 20 plates, and next to it we may rank a study by Professor Bosch-Gimpera of the affinities between Greek and Iberian art. There is also an extremely interesting article by Sr. Primitivo Gomez that gives us a remarkable survey of Iberian prehistory, and posits an unidentified and native 'Culture of the Plains' as intermediary between the upland Megalithic Culture and the Iberian Culture proper, thus filling a gap that is otherwise unaccountable. Some novel figures showing how dolmens were built add to the value of the paper, which is entitled 'Un "Hiatus" prehistorico'. Other articles deal with earlier periods; the 'aeneolithic' is well represented both in antiquities and skeletal material (note especially the sepulchral site of Cami Real), and there is also an account by Abbé Breuil of a Le Moustier industry (roughish quartzite implements) found by him near Mineteda. Notes, reviews of books, and a really good index complete the volume. T. D. KENDRICK.

Reviews

EARLY MAN IN NORTH-EAST YORKSHIRE. By FRANK ELGEE. *Printed for the author by John Bellows, Gloucester, 1930. pp. xvi, 259, with 29 plates, 67 figs. and maps. 25s.*

In 1927 we spent a holiday in northeast Yorkshire. It was a complete failure, partly by reason of the weather and unsuitable lodgings, but largely on account of the lack of any good guide to the archaeological features of the district. How different things might have been if Mr Elgee's book had been in our hands then! It would have completely supplied the need, though nothing could have remedied the weather.

This book is the result of many years, not only of patient research into the doings of former antiquaries, and of classifying museum material all over the north of England, but more particularly of careful field-work performed by one who knows and loves every detail of the countryside with which he deals. The author is an enthusiast in the best sense of that term, but his enthusiasm is controlled by common sense which is the essence of scientific method. These two qualities, together with the necessary knowledge which he possesses, combine to give his book a freshness and human interest that is lacking in so many otherwise well written works.

It is a great discredit to the reading public that such excellent topographical studies as this should have to be printed at the author's own risk, presumably on the publishers' plea that dealing with a limited area the book must be of 'merely local interest'. When will publishers realize that lessons learnt in one area are generally applicable in others? No British archaeologist can afford to neglect such a systematic topographical study as this, for its lessons are numerous and most enlightening.

The author summarizes and analyzes a vast amount of museum material collected by earlier workers, whose work he also describes. This, together with his own field-work, forms the basis of a study which is (as it should always be) four-dimensional: the dimensions are—plane topography (two), altitude (one) and chronology (one). Thus the district is described in relation to its geological zones—plains, limestone hills, high moors, etc.—and the distribution of population in each of the periods and sub-periods is plotted with very illuminating results.

A relatively large part of the book is devoted to a study of the Bronze Age, which is not surprising in a district where barrows are numbered by tens of thousands, very many of which have been excavated. The author has most to say about the middle Bronze Age, which he calls the Urn Period, and he shows that the urn-people were the only ones who ever inhabited the high moors, and he assumes that this can only have been the result of pressure exerted by more enlightened or more fortunate races who occupied the more genial lands in the neighbourhood. This leads him to believe that the urn-people may have survived in Blackmore throughout the Iron Age and even into the Roman period. This may possibly have been so, but whether the urn culture survived too, is another matter. One of the most valuable features of the book is the systematic study of the distribution of this urn culture, especially in view of the scarcity

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of middle Bronze Age habitation-sites in Britain. Not only does the author succeed in tracing numerous settlements marked by barrow-groups, cross-ridge dykes, walled enclosures, and irregular lynched cultivations, but he is even able to attribute to them one hill-fort, and a second by inference. These are all points of supreme interest. He believes that while the great chiefs were cremated and inurned in great barrows aligned beside the highways, the common herd were inhumed in large barrow-groups near the settlements. His distribution-maps of some of these groups are very suggestive, as also is the association with them of cross-ridge dykes in some instances. It suggests that our cross-ridge dykes in the south of England may possibly mark the sites of middle Bronze Age settlements also. The walled enclosures and irregular lynched cultivation-plots closely resemble many described on Dartmoor, and closer dating of the associated settlements by excavation will add materially to our knowledge of ancient agriculture. Finally, if the Eston Nab hill-fort is really to be ascribed to the urn period, as certainly seems to be the case, judging from the account of its excavation, then it must be the first hill-fort to be ascribed to the Bronze Age in the whole of Britain, and we shall have to revise our views which claim that all hill-forts are either Neolithic or Iron Age or later.

The author claims that modern farms are frequently found adjoining sites of settlements of the urn-people in the dales, and regards them as direct descendants. If this is the case, then the intermediate links in the chain of descent should be traceable. Many hollow-ways are found near these settlements and are claimed as contemporary. Our experience in the chalk country in the south leads us to hold the view that hollow-ways are seldom really old, and that they are usually the result of medieval pack-horse traffic. Besides, to judge from the plans some of these hollow-ways scarcely seem to form an integral part of the settlement in every case.

Among the other interesting points raised are :—the occurrence of horse in long barrows ; the arrangement of small barrow-groups in recurrent patterns—but does this justify a theory of star-worship ? ; the relegation of cist-burial to the early Bronze Age ; and the occurrence of leaf-shaped and even *petit tranchet* arrowheads with burials of the urn period.

The late Bronze and early Iron Ages and the Roman period are dealt with on equally interesting lines ; the author contends that the Brigantes were descended from the late Bronze Age invaders from Switzerland, while the Parisi were La Tène I immigrants from the Seine region in Gaul.

From the distribution of Celtic place-names he argues that during the Anglian settlement in the Wolds the fugitive Britons occupied the moors and dales. This is likely enough, but we could wish that the author had limited himself to place-names whose Celtic origin is vouched for by the English Place-Name Society's volume on the North Riding, remembering that Celtic river-names prove nothing as to distribution of population, and hill-names almost as little. The attempt to prove that the Brigantes spoke a Gaelic language is very unconvincing and might well have been omitted.

Cultivations of Celtic type have been traced in various parts. We hope that this will lead to their wider study in the north of England and Scotland. In connexion with these the author speaks of a certain hollow as 'certainly a dewpond' (p. 218). In view of the lack of evidence that the principles governing dew-ponds were known before the 18th century, this sounds interesting—or can it be that the term is here used wrongly for a catchment-pond of some sort ?

As a whole the book is well and interestingly written, though it might perhaps have

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been somewhat condensed without material loss. It is well produced and clearly printed, and the printer's errors are very few (p. 195, transpose footnotes). The illustrations, both photographs and drawings, are excellent, with two exceptions: the tiny diagrammatic map of northeast Yorkshire on p. xvi is not only inadequate, it is hopeless; and conveys no meaning to a stranger to the district. Also the plans on p. 141 have not been drawn with a view to so much reduction; they should have been done by an expert draftsman who knew the size to which they were to be reduced. A copious bibliography and good index complete a most praiseworthy book.

E. CECIL CURWEN.

SCHACHTGRÄBER VON MYKENAI: Text (1 Theil): Tafeln. By GEORG KARO.

München: F. Brückmann, 1930. pp. 172, with illustrations in text and 175 plates in portfolio. 120 marks.

In the autumn of 1876 Heinrich Schliemann discovered the 'Shaft-graves' within the citadel of Mycenae, the traditional burial-place of Agamemnon and his murdered companions. Schliemann's methods, even after several seasons on the traditional site of Troy, were crude, but he had the good luck to find unmistakable clues at the outset, in the shape of carved gravestones within the famous 'circle' of erect slabs, and a built 'altar', which had to be destroyed, unfortunately, as excavation went forward. For this stage in the work, the note-books of the Greek overseer, Stamatakis, rediscovered only recently in the Nauplia Museum, are important evidence, complementary to the recent careful re-examination of the whole site by Mr Wace and his colleagues of the British School of Archaeology at Athens. When the 'Shaft-graves' themselves were reached, their immense wealth of equipment had to be disengaged by Schliemann and his devoted wife, almost unaided, continually interrupted, in rainy weather, confronted daily with fresh problems of extraction and conservation, and unprovided even with the most elementary kit of the modern excavator. What wonder that an answer is not now to be had to many questions suggested by fifty years of intense study and of skilled dissection of many sites which both illustrate the 'Shaft-graves' and at the same time reveal our lack of precise information about them? Only in graves II and V which were opened latest, was the position of individual objects noted; and only grave VI, discovered a year later by Stamatakis, has been reassembled, as it lay, in the National Museum. Schliemann's own book, *Mycenae*, was published in 1878, and the *Mykenische Thongefässe* of Furtwängler and Loeschcke in the same year: the latter far the finest study of prehistoric pottery that had appeared at all. Then follows a curious pause, while Schliemann excavated Tiryns in 1884-5, and an increasing number of other workers were attracted into this new field of 'Mycenae' studies. In the nineties, Belger minutely examined the site, Reichel began to describe the museum-objects, Gilliéron's electrotypes popularized a few of the most notable; but it was long before we had even an adequate handbook to the show-cases, from their custodian Stais. Then after 1900 came the revolutionary discoveries of Sir Arthur Evans and others in Crete; and Mycenae, no longer either immemorial or central, had to wait its turn, for sounder, but by no means adequate reasons. It was only about 1912, at the instigation of the veteran Loeschcke, that Dr Karo undertook to photograph and describe systematically the whole 'Mycenae collection'; and then came the War. Dr Karo's preliminary survey, already partly in type for the *Athenische Mittheilungen*, vol. XL (1915) was not completed till 1927, though by an act of 'singular liberality' proof-sheets were transmitted to Sir Arthur Evans in 1915 (*The Shaft Graves and Beehive Tombs of Mycenae*, 1929). But it has taken another decade to bring this splendid publication to its present stage.

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Naturally, with all respect to Dr Karo's text, one turns first to the plates. These could not be better. All objects which could be photographed with any useful results are represented, on full scale whenever they are not too large for the quite ample page, and each with its tomb-reference and catalogue number. Now at last we can really *quote* Mycenae. Of some quite fragmentary objects there are the skilful drawings of the elder Gilliéron, and a few by the son who inherits so much of his gift. Very wisely, the well-known 'cat-dagger' and 'lion-dagger' with their many-coloured alloys, are only represented by photographs, and amazingly well: no tinted picture can really recall their peculiar charm.

The present instalment of text includes a brief narrative of the discovery, and a minute description of the shape and surroundings of the 'Shaft-graves' themselves, in which good use is made of the plan and detailed observation of the British School, and a chronological perspective of this part of the site of Mycenae is attempted, very cautiously, for as Dr Karo says 'even Tiryns throws no light' on the question how the 'Shaft-graves' lay in relation to other buildings, and especially to earlier graves of which the remnants surround them. What is clear (p. 17) is that they mark a 'profound change' which 'rests obviously on a different idea of duty towards the dead', and therefore open a new period, of which they are still the only witnesses. For Dr Karo regards the 'Shaft-graves' as predecessors of the 'Beehive graves' which he assigns to the years about 1500 B.C. (p. 19), quoting as permissible Wace's suggestion that they mark the arrival of a new dynasty, though (he thinks) not a shift of population. Consequently the old burial place was simply disused, after six graves, or so, had been installed. After the Fall of Cnossus, about 1400 B.C., new political conditions increased the wealth and importance of Mycenae; and also the risks of power during a rule of force. To the next generations belong the new palace, the citadel wall with its 'Lion Gate', the contemporary 'Circle' to delimit and protect the old cemetery, and a re-erection of the original gravestones at a higher level resulting from deliberate infilling of the 'Circle'. Separate sections here deal with these 'Stelae', including the numerous fragments recently published by Mr Heurtley (*British School Annual*, xxv, 126-46); and with the fundamental question of the arrangement of the bodies and their equipment within each grave. Dr Karo is satisfied that they lay directly on the shingle floor, and were so deposited in the first instance. This leads (p. 40) to an important note on the different view of Sir Arthur Evans (*Shaft Graves and Beehive Tombs*) that the 'Shaft-graves' contain 'secondary burials' of bodies removed for safety from the 'Beehives' in the Lower Town; but detailed criticism of this view, and of the relations between Mycenaean and Cnossian art which it implies, is reserved for the second instalment of Dr Karo's text.

Of the detailed descriptive inventory, it is sufficient here to say that it supplies what we have all needed so long: an exact account of each object, under its inventory number, with measurements and references to these plates, to Schliemann's line-drawings in *Mycenae*, and to subsequent publications elsewhere. Not least important is the record of attempted 'restorations' which on some of the most important gold work have been carried rather far. A number of photographs and drawings in the text of the inventory supplement the plates in regard to certain objects of minor importance or special descriptive interest.

The appearance of Dr Karo's own interpretation of this amazing collection will be awaited with all the greater interest, now that he has made it possible so conveniently and thoroughly to refresh our memories of the material.

JOHN L. MYRES.

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SEVENTY YEARS IN ARCHAEOLOGY. By Professor Sir FLINDERS PETRIE.
Sampson Low, 1931. pp. 284, illus. 18s.

This is the autobiography of an illustrious pioneer who belongs to the heroic age of archaeology. Born in 1853 Petrie began his career in England, hiking about the country planning earthworks and stone circles. Upon this solid foundation was laid his more ambitious survey-work in Egypt, which gradually developed into excavation. It was in Egypt that his greatest work was done, demonstrating by practice and invention that archaeology has a technique of its own. Petrie was the first scientific excavator in Egypt, and he naturally collided therefore with the French archaeologists, who held (and still hold) the highest official posts. Priest and prophet are always at war, for prophets are born to vex the fat slumbers of the great, and it is their mission to gird at established institutions. Our sympathies are with the prophets; but it is well to remember that, like God, they have written all the books; we get therefore only one side of the case. In addition, allowances must always be made for the idiosyncracies of a strong character.

There are many interesting sidelights on modern Egypt and on its exploitation. The stories on pages 185, 186 and 192 are particularly illuminating, and cause us to ask whether the time has not come for the fellah to take charge of his own country. It is at any rate a fact that in every book on Egypt he always shines in contrast with the European and the effendi; and this impression is confirmed by the experience of those who know him and his land.

The book is a striking testimonial to the gospel of work, and to the reward it obtains. Some of the author's opinions are quite properly to be regarded as revolutionary, especially those given on pages 126, 133, 145, 154, and 221. They would be applauded in certain quarters with an enthusiasm that might both surprise and embarrass him.

There are two photographs of the author; though good, neither of them in our opinion is a really first-rate portrait.

The book should be read by all interested in the history of archaeology.

ANATOLIA THROUGH THE AGES: DISCOVERIES AT THE ALISHAR MOUND, 1927-29. By ERICH SCHMIDT. *Oriental Institute of Chicago Communications*, no. 11, 1931. pp. 165. 2 dollars.

On the archaeological map Anatolia is denoted by a virtually blank patch interposed between the ancient civilizations of Mesopotamia and the younger ones of the Aegean and the Balkans. Excavations undertaken at a few points on its western fringe have disclosed a peculiar culture, still ill-defined in time, but obviously related to the Helladic, Cycladic and Minoan civilizations in the Aegean and, more vaguely, to the Vinča, Lengyel and Danubian cultures of Central Europe. The treasures of Troy demonstrate the importance of the region at some time as a centre from which metal types were transmitted towards Europe. Yet the types in question have been directly or indirectly inspired from Mesopotamia, and in seeking the mechanism of their westward transmission we cast hungry eyes upon the Anatolian hinterland, traversed in classical times by the Persians' Royal Road to Susa. From the eastern confines of the plateau stray vases have trickled into European museums as the result of clandestine diggings or even less scientific 'tablet-hunts' by philologists. Some point to Troy and the West, others have different affinities, but, despite penetrating stylistic and technical studies by Myres,

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Frankfort and de Genouillac, attempts at their classification and appreciation are left rather in the air through lack of stratigraphical data.

Even from the brief and extremely popular summary before us we can see that the American excavations at Alishar, between the imperial Hittite capital of Boghaz Keui and Kul Tepe, famed as the source of the Cappadocian tablets, will automatically coordinate such scattered fragments and define with new precision the relations between Mesopotamia and the West.

The black carboniferous wares, which from Aegean analogies we should expect at the base of every culture in this area, had only just been reached as the volume went to the press and are described in an appendix as 'Neolithic'. The ten-metre deep deposit containing them is overlaid by an accumulation of comparable thickness formed by the debris of a 'Copper Age' town, already fortified and frequently rebuilt. Here burnished red-slip ware predominates with many forms reminiscent of Troy and Thermi. And, as there, we find perforated stone battle-axes, grooved hammer-stones so important everywhere in the history of early metallurgy, conventional fiddle-shaped idols and metal pins. The citizens, buried in huge jars, as at Yortan, were not 'Alpine' nor 'Armenoid', but mesaticephalic and slightly prognathous.

The 'Copper Age' culture passes directly into that of the early Bronze Age, termed Alishar III in the body of the report. To this phase belong the well-known Cappadocian vases, still Anatolian in form but now painted with black patterns on a red ground. Lozenges are the only motives illustrated, but the forms are identical with those from Kul Tepe in the Louvre which bear the little pot-hook spiral that Dr Frankfort derives from Macedonia and calls Phrygian. This group is consequently made several centuries earlier than that author had placed it. But during this early Bronze Age, part of the site at least ('Alishar II' in the text) was occupied by an 'Alien People' definitely brachycephalic, and using the potter's wheel. In their settlements were found tablets of the familiar 'Cappadocian' type, records of Semitic merchants settled in the region and trading with Assur and even Ur during the centuries round about 2000 B.C. Schmidt attributes to these Semitic colonists the advances in pottery and metallurgy that characterize his Alishar II, but von der Osten in his appendix identifies the Alien People with the Hittites of the 'Early Empire' which lasted till about 1650 B.C. He notes that the pottery types introduced by the 'Aliens' are not Mesopotamian (the metal types on the other hand are); nor are they European, but Anatolian or North Syrian with Minoan affinities (metallic-looking spouted pots and goblets with crinkled rims). In any case the Aliens may be held responsible for an intensified activity in metallurgy, and significantly enough we find in their settlement many types familiar at Troy and further west: thus there are lead idols like the well-known specimen from Troy II; a vase with a figure in relief on its walls forms a link between an almost identical sherd from Csoka in Hungary and others from Assur; mysterious flat 'cakes' of clay in the form of segments of a circle pierced at either end were apparently common. Siret has found just such objects on Copper Age sites in the metalliferous regions of southeastern Spain and has proposed to reconstruct a primitive reverberatory furnace out of them. It really looks as if the activities of the civilized aliens in Cappadocia may have had repercussions far afield in Europe. With the aid of Hittite and Assyrian records Alishar II should be precisely datable, and in turn assist an accurate dating of the European Bronze Age. At the moment its chronological limits are not clear. Alishar IV, which eventually replaces it, should coincide with the New Hittite Empire, 1400-1200 B.C., and witnesses apparently the rise of iron-working and the adoption of the fibula in an

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advanced form—too advanced it would seem for 1200 B.C. Yet the upper limit 1400 leaves an uncomfortable gap between the Cappadocian tablets in Alishar II. For the resolution of such ambiguities we must await the completion of the excavation and its final publication. It has nevertheless seemed worth while even now to signalize the possibilities implicit in the work in progress.

V. GORDON CHILDE.

DIE AUSGRABUNGEN VON SAMARRA, band v: Die Vorgeschichtlichen Topfereien von Samarra. By ERNST HERZFELD. Berlin: Verlag Dietrich Reimer-Ernst Vohsen, 1930. pp. VII, 110, 240 text-figures and 47 plates. £5 5s.

With a wealth of figures and collotype plates, six of which are coloured, the early pottery found at Samarra is here published. The late Dr Hall (*Journal of the Royal Asiatic Society, Centenary Supplement*, 112) and the present reviewer (*Studies in Early Pottery of the Near East*, I, 60 ff) had in 1924 assigned this fabric to its proper context amongst the homologues of Susa I and Al Ubaid. Their conclusions are now adopted by Professor Herzfeld, who does not deem it necessary however to refer either to the earlier research on the subject or to the divergent view published by the head of the Samarra expedition, Professor Sarre (*Der Islam*, v, 190 ff), which was based on an interpretation of certain imported North Syrian sherds as Aegean. In the case of these Professor Herzfeld again endorses without mentioning it the more correct suggestion made before.

The conditions under which this very remarkable pottery was found are now for the first time fully published. It derives from a cemetery which occupied a layer of earth of 1.50 m. thickness, wedged in between virgin soil and Arab houses of the 9th century A.D., which were found immediately above the graves of the fourth millennium B.C. To this circumstance is due the occurrence of a square peg of non-meteoric iron in one of the graves, and of some Arab glass in another. The graves were sometimes lined with sun-dried mud-brick or with stamped mud. Their orientation was not uniform. The bodies were found lying on the back and extended, as at Ur in the Al Ubaid period, but different, apparently, from the usage at Susa: there people were buried in the crouched position. Some simple alabaster cups were found, and beads of limestone, turquoise, carnelian, and shell; in one case there were more than 3500 beads in one grave so that Herzfeld thinks of a beadwork cover for the whole body. A globular stone macehead and some microlithic implements were also found.

Important is the occurrence, furthermore, of some small copper tools and of some bowls of hammered sheet-copper. The vast majority of the finds consists of painted pots. The shapes are more primitive than those from Al Ubaid; there are no bases, rims or handles. Shapes and designs take an intermediate position between Susa I and Al Ubaid, and are parallel with Tepe Khazineh, near Tepe Musyan; but the rich variety of all these early wares gives a character of its own to the fabric of each site within a more or less greater province of homogeneous culture. Unequalled elsewhere is the bowl-and-stand made in one piece, which recalls similar vases known in the early dynastic Sumerian period from Assur H and G, and from Kish and Ur; but it is impossible to say, and perhaps improbable, that there is any real connexion or continuity, as we cannot trace this type of vessel through the intervening Uruk and Jemdet Nasr periods.

The exceptionally interesting designs of the vases are, often from minute fragments, restored in excellent drawings; and we see radial compositions in the bowls showing women with flowing hair; and scorpions, birds and fishes; and tête-bêche groupings of goats, all combined with purely geometrical motives. Professor Herzfeld's comparisons

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of his discoveries with those of others fall far short of what one has a right to expect on the strength of the list of literature given in a note to the introduction. They remain vague, and limited in scope. Important however are a number of incidental remarks on pottery picked up in Northern Persia and at Nehavend, and at Persepolis; some specimens are shown on the plates. A publication of this material, in a less beautiful and therefore less costly form, would be a great service which Professor Herzfeld could render to science without much trouble. For this material bears directly on the problems connected with the earliest civilization of Mesopotamia as we shall endeavour to show elsewhere.

H. FRANKFORT.

THE TEMPLE OF THE TOOTH IN KANDY. By A. M. HOCART. *Memoirs of the Archaeological Survey of Ceylon*, vol. IV. Luzac and Co., 1931.

This is a most valuable contribution to the archaeology of Ceylon. It also has great significance for all anthropologists. Mr Hocart says in his preface: 'a temple is merely a receptacle for worship, and can therefore only be properly understood in the light of the ritual that goes on inside. To neglect our opportunity of acquainting ourselves with that ritual is like studying molluscs solely from their shells'. The archaeological survey of the temple is illustrated by detailed drawings and well reproduced photographs, and its ceremonies and personnel are equally well dealt with. Here at last is set down southern Buddhism as it *is*, not as it is supposed to be in various texts of mixed origin and authenticity. Indian architecture and sculpture will only begin to be understood when comparable work is done for the great temples in the South, the Deccan, Gujarat and Orissa. Living tradition is clearly better, as a guide to real understanding, than contradictory texts, which are difficult to date and locate.

Mr Hocart's retirement is a great loss to the Archaeological Survey of Ceylon. It is to be hoped that the tradition he has built up will be maintained.

K. DE B. CODRINGTON.

THE KADAMBA KULA. By GEORGE MORAES. *Studies on Indian History of the Indian Historical Research Institute. St. Xavier's College, Bombay.* No. 5. Bombay: B. X. Furtado and Sons. [London agents, Luzac and Co.] 1931. 25s.

Father Heras introduces this volume and as Director of the Institute he may well be proud of it. In a country as big as India generalizations of any kind are dangerous. Vincent Smith compiled the first sequent history of India, but his single volume is necessarily full of generalizations. The time is now ripe for a series of dynastic monographs which will provide the detailed structure for another general history. The lack of such works as this is the direct cause of the unsatisfactory looseness and one-sidedness of the recently issued volume of the Cambridge History. The Institute, Father Heras and the author are to be congratulated upon pioneer work in what is virtually a new field. The geographical basis of the work is especially welcome.

K. DE B. CODRINGTON.

A HOARD OF BRONZE IMPLEMENTS FROM DONHEAD ST. MARY AND A STONE MOULD FROM BULFORD, IN FARNHAM MUSEUM, DORSET. By A. D. PASSMORE. *Wilts Arch. Mag.* June 1931, XLV, 373-6, 3 plates.

Students of the late Bronze Age and its many intriguing problems of trade and invasion will welcome this detailed description of a hoard found 36 years ago in a remote

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part of Wiltshire. General Pitt-Rivers, who acquired it, visited the spot and found a piece of coiled bronze wire overlooked by the finders; but he died before he was able to publish any account of it (as surely he would have done). Our thanks are due to Mr Passmore for thus making it known as it deserves. The hoard consists of 8 winged palstaves of the type whose European distribution is shown by the map in *ANTIQUITY* (iv, 159, illustrating a paper on 'The Sword Bearers' by Mr Estyn Evans). It also included other objects, amongst them being a mould for a straight-edged socketed axe. We take this opportunity of calling attention to a later article by Mr Evans (published in *MAN*, xxxi, 208) where the same map is published again, and where an attempt is made to run these late Bronze Age invaders to earth in their Central European lair.

AN INTRODUCTION TO PHYSICAL ANTHROPOLOGY. By E. P. STIBBE.
Edward Arnold, 1930. pp. vi, 200, with illustrations and one coloured map. 12s 6d.

This book states that it is meant for students who intend to take a degree in the University of London, and follows the syllabus. His intention apparently forces the author to write something which reads very like the sort of note-book that a first-rate student would make if he attended and understood all the lectures and demonstrations given during the course. Such a work would be of great value to such a student, representing as it does the digestion and tabulation of things heard, seen, and done in the laboratory and lecture room, and would serve as a mnemonic for examination purposes or as a convenient abridgment of necessary facts for future reference. But it is hard to see the value of the book to one who has not heard the lectures, seen the demonstrations, and worked in the laboratory or field. None of the material is really intelligible to the man who has not handled or studied specimens for himself, and in the very nature of the subject-matter could hardly be so. The facts in the book are sound enough, no doubt, but they mean little to the man who has not actually gone through the work represented and found them out for himself. If he cannot do such work, he had better leave the subject alone, for it is decidedly one for the specialist, and not one which a man can work up in a library.

T. K. PENNIMAN.

A SHORT COURSE ON PHYSICAL ANTHROPOLOGY. By M. R. DRENNAN.
Cape Town: Mercantile Press. pp. 60 and numerous illustrations, unbound. 5s 6d.

This little book is intended for the use of the medical student and graduate in South Africa, and for this reason a large amount of space is given to a discussion of the races of that region. Considerable use is made of leaded type and the general arrangement rather suggests that the book is meant for those who study anthropology for a specific purpose, such as perhaps an examination, than as a scientific pursuit. This is a pity as it has undoubtedly cramped the author, whose work on the anthropology of South Africa is well known. At present unfortunately there is by no means a universal agreement as to the method by which anthropological measurements should be made. The author however gives no critical discussion of method and in some cases suggests methods which many anthropologists, even in this country, do not use. In the case of the living he does not even define the majority of the measurements suggested. It is probable that these deficiencies are due chiefly to the extreme compression from which the book has suffered and it is to be hoped that the 'short course' will be expanded into a critical discussion of the methods and problems which his space here only allows him to state dogmatically.

L. H. DUDLEY BUXTON.

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SCOTTISH PLACE-NAMES. By W. C. MACKENZIE. London: Kegan Paul, Trench, Trubner and Co., 1931. pp. 320. 15s.

This is the first book to deal with the subject in general since the publication in 1892 of the Rev. J. B. Johnston's *Place-Names of Scotland*. In the interval a few works on individual counties or special areas have appeared and the Celtic element in particular has been very thoroughly investigated by Professor Watson. The Norse side has been dealt with in some of the papers of the late Dr MacBain, but no serious discussion of the English names has as yet been attempted by any competent scholar.

Mr Mackenzie's book is not a mere index of names. It consists of a series of chapters in which the names are grouped topographically under the various headings of hills, islands, lochs and habitations, etc., and in conclusion the much-discussed Pictish question is again brought forward, though without any definite further result. At the end of each chapter the more important or interesting names occurring in the text are discussed at greater length with their early spellings, though, owing presumably to lack of space, the author has in most cases omitted to state the sources from which his material has been extracted.

It is perhaps a pity that Mr Mackenzie did not find space also for a chapter dealing with the proportion of names of Gaelic, British, Norse and English origin in the different counties, and illustrated by one or more maps. From a superficial examination of the book it would appear that English names are confined chiefly to the southeast, though isolated examples occur elsewhere due to the gradual spread of the language in the course of centuries. As in England there are a few interesting cases of corruptions or of folk-etymology, e.g. Oxton for original *Ulfkilston* and Channelkirk for earlier *Childenekirk*. It is doubtful if Hassendean stands for 'Alstone's dean or wooded valley'. Other early spellings from the Melrose cartulary are *Hatstaneden*, *Hadestanden*. Jedburgh no doubt contains a river-name as first element but this is unlikely to be of Norse origin. Corstorphine may contain a hill-name as second element but the interpretation 'Thorfin's cross', which has been put forward by other scholars, seems more likely.

Norse names occur chiefly in the islands and in Caithness and Sutherland and on the western mainland from Cape Wrath to Solway. The Beaully Firth was formerly thought to be their southernmost limit on the east coast, but the author notes that Moorfoot in Lothian was *Morthwait* in the 12th century. In the west the Norse names have been much corrupted through Gaelic influence, *vik*, 'bay', commonly appearing as *-aig*, and *fiqrör*, 'fiord', as *-ord*, *-ort* and *-art*. It is doubtful if Reay and the first syllable of Caithness are to be taken as Norse. The former is more likely to be connected with the Gaelic *ràth*, 'fort', and the latter is no doubt a tribal name from the Gaelic *cat*, 'cat'. On the other hand the first element of Grumbeg, earlier *Gnowb*, is clearly Scandinavian from ON *gnúpr* 'peak', 'summit'. Kirkwall is rightly explained as 'church bay' but the author is hardly correct in stating that 'it is phonetically impossible for *vagr* to become *wall*'; for a similar phonetic development is evidenced in many English place-names.

As in England, names of French origin are rare except in one or two instances of abbeys of medieval foundation. Montrose is *Munros* a.1200 containing the Gaelic *monadh*, 'hill'. A few Norman-French personal names occur as first elements of place-names, usually with the OE suffix *tun*.

The number of places containing a saint's name are very numerous but probably not more so than in Cornwall and other Celtic areas. A note on the distribution of place-names in *Kil-* and *Kirk-* would have been of interest.

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Of Celtic names perhaps too large a number have been assigned to Gaelic rather than to Pictish or British origin, but the whole subject, as shown in Professor Watson's book, is one of great difficulty. The Pictish element is naturally strongest in eastern Scotland north of the Forth, and the British element in south Scotland including Lothian. It is clear from the evidence of early records that in many cases Pictish names have been ousted by later Gaelic ones. *Baile*, for example, has been substituted for *Pit* in many instances.

In Scotland, much more than is the case in England, one would look for a certain number of pre-Celtic names, especially among the names of rivers and larger islands. Rum and Mull have been cited as examples by some writers, but Mr Mackenzie would prefer Gaelic origin even for these. Intensive study of the place-names of individual counties may yet bring to light more names of very ancient origin.

Taking into account the size of the book and the size of the area which it covers, the number of interesting and well-known names which have been brought together is surprisingly large. At the same time it is clear that many difficult problems must remain unsolved until the toponymy of Scotland has been investigated, county by county, after the same thorough fashion as that adopted by the English Place-Name Society for the Survey of English Place-Names.

J. E. GOVER.

DIE JUNGNEOLITHISCHEN KULTUREN IN WESTDEUTSCHLAND. By R. STAMPFUSS. Vol. II of the *Rheinische Siedlungsgeschichte*. Ludwig Rohrscheid, Bonn, 1929. pp. 229 with 44 illustrations in text, together with an album of 14 plates. 25 R.M. paper cover; 30 R.M. bound.

If unfortunately many of the prehistoric cultures of the mainland have reached this island in an attenuated and poorer form, we have at least in the beaker-culture a brilliant exception to the general rule, an exception which we owe to our position opposite the mouths of the Rhine. The virility and wealth of variety present in the beaker-culture of Britain is due to the fact that our invaders took off from that veritable Babel of beaker-folk, the lower reaches of the Rhine. Dr Stampfuss treats with great clarity of the main beaker-cultures of the Rhine area, distinguishing the Corded beaker-folk with their amphorae and faceted battle axes, the single grave-folk of the first two phases, the bell-beaker intruders, and finally the 'Zonenbecher' group representing a fusion of the corded and bell-beaker cultures. It is the description of the formation of this last group in the Rhine Valley, its progress down the river, and its contacts with the westernmost extensions of the Single Grave-folk in the Low Countries before setting out to our shores that will prove of greatest interest to the average English reader. Points of detail of especial domestic interest are comparisons between individual beakers from Britain and the Continent e.g. the Beakers from Felixstowe (Aberc. 86) and Exloo (illustrated *ANTIQUITY*, December 1931, v, 415). An interesting illustration (no. 22) compares the profiles in section of selected British and Rhenish beakers, revealing for instance that the bevelled lip, thought by some to be peculiarly English, and the raised rib ornament on the neck are both found on pots from the Rhine and the Netherlands. Another interesting fact is that the makers of the 'Zonenbecher' had on the Continent a pleasant habit of polishing their flint knives (p. 86).

Stampfuss insists that the bell-beaker complex of the Rhineland is quite distinct from the other beaker-making cultures in the area, and secondly that the Palatinate group is the earliest in Central Europe. He accepts the Iberian origin of the bell-beaker,

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and by implication the independent origin of the Cord-Single-Grave group since the bell-beaker only reached the Rhine in stage 2 of the Schnurkeramik.

Stampfuss makes good use of the geographical method as well as of the typological, mapping both different types of pot and typical associated objects, thus showing up well the zones of the various groups, areas of fusion and so on. He also gives us a chapter on methods of burial, and another on the beaker-cultures as the foundation of the early Bronze Age. The album of plates covers a large number of specimens in the manner of Abercromby. Finally one must mention the very complete indices as models of their kind. This is a book essential to the student of the British beaker-culture for its treatment of origins. The remarks on the British pottery itself, which the author has not examined at first hand, are not very valuable. J. G. D. CLARK.

OFFA'S DYKE: A FIELD SURVEY. SIXTH REPORT: OFFA'S DYKE IN THE WYE VALLEY. By CYRIL FOX and D. W. PHILLIPS. *Archaeologia Cambrensis* June 1931, LXXXVI, separate pagination.

With this report the Director of the National Museum of Wales and his collaborator conclude the most important survey yet carried out in this country of a typical 'linear earthwork'. Indeed, if we leave out of account the work done on such complex affairs as the Roman limites, this survey of Offa's dyke is the most careful and complete of its kind ever carried out anywhere. We wish that similar surveys could be made of some of the German, Transylvanian and Roumanian boundary dykes, to which Sir James Berry called attention twelve years ago (*Geographical Journal*, March 1919, pp. 129-52); as well as of the mysterious 'Median Wall' in the Tigris Valley, the Sadd-i-Sikandar near Astarabad, and the Merv rampart near Tejend in Turkmenistan. But in the absence of any large-scale maps, such as is provided here by the 25 inch to the mile Ordnance Survey Maps, any such attempt would only be preliminary. (We mention the above examples in the hope that some reader, contemplating a holiday tramp abroad, may turn his attention to them).

There are still of course fields to conquer at home. Watt's Dyke badly needs similar treatment, and Dr Fox is providing it. And there are many others, like the Scots Dyke and the Roman Rig in Yorkshire, which we intend to tackle ourselves (on the 6 inch scale) if no one else will.

In the course of the survey Dr Fox has made many new and valuable discoveries, such as the relation between the alignment of Offa's Dyke and the nature of the vegetation at the time of its construction. It would be a boon to all investigators if he would republish these reports in book form. In the hope that he may do so we have postponed anything of the nature of a full summary or review of the parts as they have appeared.

O.G.S.C.

THE 'SANCTUARY' ON OVERTON HILL, NEAR AVEBURY. By M. E. CUNNINGTON; being an account of excavations carried out by Mr and Mrs B. H. Cunnington in 1930. *Wilts Arch. Mag.* June 1931, XLV, 300-35. *Ten plates, including 2 large plans.*

The *Wiltshire Archaeological and Natural History Magazine* occupies a very high place amongst county archaeological journals, just as Wiltshire has more important prehistoric sites (one might almost add, more archaeologists) than any other English county. No student of British prehistory can afford to miss reading each issue of the magazine, for it is sure to contain something of general importance. It is not perhaps

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easily accessible except to members, but that is a good reason for joining the Wiltshire Archaeological Society whose headquarters are at the Devizes Museum (annual subscription 15s 6d, entrance fee 10s 6d). We make no apology for drawing the attention of our readers to the Society, particularly of librarians at home and abroad, for it has behind it a long record of brilliant work, and the present number gives promise of an equally distinguished future ahead of it.

The outstanding feature of the present number is the account of Mr and Mrs Cunnington's excavation of a composite circle of stone and wooden pillars at the east end of the Avebury Avenue. The exact site of this monument had been forgotten, though it was recorded approximately on the Ordnance Maps. It was rediscovered by trenching, and the stone- and post-holes cleaned out. We do not propose to attempt to give any summary of the results (one of which was described in *ANTIQUITY*, 1930, v, 233-4, by Mrs Cunnington). They are admirably described and discussed in the present paper, which is illustrated not only by photographs and drawings (by Mr Gurd, who now needs no introduction to archaeologists) but also by two splendid plans in colour. Comparisons are odious and we refrain from the temptation to make them; but we will say that we in this part of England do these things better, from A to Z, than others.

EASTON DOWN, WINTERSLOW, S. WILTS; FLINT-MINE EXCAVATION,
1930. By J. F. S. STONE, B.A., D.Ph. *Wilts Arch. Mag.* June 1931, XLV, 358-65,
plan and 8 plates.

Flint mines have been discovered on the Sussex Downs, at the southern end of the Chilterns near Rotherfield Peppard, and near Brandon in Norfolk (Grimes Graves). Hitherto none has been known in Wessex, and it was always a puzzle why not. Dr Stone has provided the answer. They existed, but none of the rest of us who have tramped the downs had been lucky enough to spot them. To have discovered them, excavated two shafts completely with one's own hands, and then to have written and illustrated this concise and admirable report is a feat which speaks for itself. Dr Stone leans strongly to the hypothesis (soon probably to become an established fact) that the shafts are of Beaker age, quoting Sussex evidence in support (apart from evidence on the spot). With regard to the types of flint implements found he states that 'the site has yielded a great variety of types, which if taken singly could be ranged over a considerable period of time'. He concludes that 'form alone is an unreliable criterion of age'. We agree of course with his opinion, but we fancy that most flint-experts would also. Surely nowadays single *isolated* specimens are not dogmatically assigned to any given period of limited duration?

A SETTLEMENT SITE OF THE BEAKER PERIOD ON EASTON DOWN,
WINTERSLOW, S. WILTS. By J. F. S. STONE, B.A., D. PHIL. *Wilts Arch.*
Mag. June 1931, XLV, 366-72, 4 plates.

Dr Stone discovered and excavated with his own hands this, the first 'pure' beaker habitation-site to be discovered in Wessex. It occupies the same piece of downland as the flint-mines (*see above*); remains of any other period are negligible; and there seems no reasonable doubt that the shallow pits he cleared out were the homes, perhaps seasonal, of the miners. The article is illustrated by excellent drawings and plans, made by the writer himself; and though this is merely a preliminary report the site is of more than merely local importance. O.G.S.C.

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DER TELL HALAF: eine neue Kultur im ältesten Mesopotamien. By MAX VON OPPENHEIM. *Leipzig: Brockhaus, 1931. 14 marks.*

Baron von Oppenheim has here given us an interesting account of the important excavations he has been carrying on since 1911 at Tell Halaf, east of Harran, on the banks of the upper Khabur. They have brought to light a large number of monuments and other remains which have revealed to us a new phase of art and civilization in ancient Mesopotamia. The earlier remains go back to the Aeneolithic period of Mesopotamian history, the age of the so-called painted pottery which has been found in Elam and Babylonia as well as in Assyria and Mesopotamia, Northern Syria and Eastern Asia Minor, and is coeval with the first two periods of Susian ceramics.

At Tell Halaf a remarkable series of sculptured stone monuments associated with the painted pottery has been discovered. In an appendix Prof. Herzfeld divides them into three periods, period I being further subdivided into 1a and 1b, extending from about 3300 B.C. to the age of Naram-Sin (2500 B.C.). Most of them are of basalt, but calcite was also used in period I. In period III the advance in art is very marked.

The preservation of the monuments is due to their having been reused when the city which had long lain waste was restored by the Aramaic prince Kapara, son of Khadiann, 'King of the land of Pa-lit(?) -ema' about 1200 B.C. His palace and temple were adorned with the statues and reliefs of the earlier epoch, though unfortunately many of them had been broken and the reliefs were embedded in the walls of the new buildings with little regard to their relationship to each other. Among the most striking are the three colossal figures of divinities standing on animals whom Baron von Oppenheim would identify with Tessub (on his ox), the Sun-god and the goddess Kheba on lions, the figure of a sphinx being stationed on either side of them. They belong to Prof. Herzfeld's period III rather than to the age of Kapara who re-erected them in the great northern façade of his palace.

Another valuable appendix on the pottery is contributed by Prof. Hubert Schmidt and among the numerous photographic plates contained in the book are two in colours depicting the typical potsherds of the prehistoric epoch. The latter will be specially appreciated by the archaeologist who is too often handicapped in his study of ancient pottery by being forced to depend on a description of its colouring in more or less ambiguous words. Prof. Schmidt draws attention to the fact that on some of the potsherds there is the picture of a horse, and notes that there are other indications of the use of the horse in Northern Syria in early days for the purpose of riding as well as of drawing a car.

The pottery of Tell Halaf exhibits a more advanced art than the sculpture. This latter in its early phases indicates a connexion with the art of early Sumer, but it soon developed on lines of its own and I am inclined to think that a good deal which is ascribed to an early date is really due to the imperfections of the provincial sculptor. On the other hand we can trace in it the origins of the so-called Hittite sculpture, so many examples of which have been discovered at Carchemish and which I should prefer to call Early North Syrian. One of its characteristics is its fondness for composite figures, partly human and partly animal, the best-known western example of which was the Chimaera. It is also distinguished by its indebtedness to the Gilgames cycle, which raises the question whether the latter really had its origin in Sumer and not rather in Northern Mesopotamia. On the one hand the seal-cylinders which represent the heroes of the cycle go back to an earlier epoch in Babylonia and Elam than anything yet found at Tell Halaf or the

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neighbouring Jebel el-Beda; on the other hand it is difficult to explain the name of Gilgames from the Sumerian vocabulary. The bilingual vocabularies it must be remembered, as I have long since pointed out, contain many words of non-Sumerian or pre-Sumerian origin which accounts for the large number of apparently Sumerian synonyms recorded in them.

The inhabitants of Tell Halaf were not a literary people, and accordingly no inscriptions have been found earlier than the reign of Kapara. Naturally when it became the capital of the Assyrian province of Guzana there was a change and a good many Assyrian and Aramaic tablets have been discovered. The Aramaic tablets are interesting as they illustrate the adaptation of the Phoenician alphabet which was intended for papyrus or leather to clay. Prof. Meissner has found among the tablets only one small literary fragment, which reproduces an exorcism; the rest are of a commercial or official character. It may be remembered that one of the inscriptions of Kapara was to be seen in the British Museum for several years after the war.

A. H. SAYCE.

THE STONE AGE CULTURES OF KENYA COLONY. By L. S. B. LEAKEY.
Cambridge University Press, 1931. pp. 283, 47 figures, 21 plates and 2 maps. 25s.

In the recent marked turn of archaeological attention to Africa the researches of Dr L. S. B. Leakey have been directive in no small measure, and it is safe to say that they will proceed to make a similar call by future continuation. In the volume under review its author provides, with admirable clearness, a survey of the results of two seasons' excavations by the East African Archaeological Expedition (1926-7 and 1928-9). As may be anticipated from the title, Dr Leakey deals especially with Stone Age cultures and sub-cultures of which he describes no less than fifteen; but in addition he has important chapters on climatic changes and on the Pleistocene fauna; while the human remains are considered together with their associated cultures. There are seven appendices dealing with geology (by J. D. Solomon), pluvial periods (C. E. P. Brooks), fossil mammalia (A. T. Hopwood), mollusca (M. Connolly), a reprint of Professor Gregory's chapter on 'Prehistoric Man' extracted, with its author's sanction, from *The Rift Valleys and Geology of East Africa*, beads (H. C. Beck), and an undated extract from the *East African Standard* dealing with the discovery of the Nakuru burial site (J. A. Macdonald). The book is all the more useful to the general reader because it achieves the purpose of an excellent outline, and we are promised the publication of specialist subjects, such as a detailed discussion of the human remains, in later works.

Palaeontological evidence for dating purposes is seriously lacking at present in Kenya, for nearly all the remains discovered are those of living species, but past periods of heavy rainfall (pluvial periods) are hypothetically equated with the Günz-Mindel and the Riss-Würm glaciations of European nomenclature, as was done with the first and second pluvials of Uganda, and with the Bühl and Sub-Atlantic phases of the higher northern latitudes—a tentative correlation by no means unjustified and one likely to receive support from further work. As in Uganda, so in Kenya, it would appear that the second major pluvial (the Gamblian) displays two maxima separated by a relatively dry spell, and in both countries Pleistocene time is punctuated by earth movement which has played a great part in moulding the topography of Eastern Central Africa, and is of peculiar significance within a great circle of country centrally occupied by Lake Victoria and bordered by the rift valleys. According to Solomon, the dry period between the first and second major pluvials was one of great volcanicity and earth

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movement manifested by faulting. In Uganda, however, the great period of rift valley expression was that of the decline of the second pluvial, or during the dry period succeeding it. Dr Leakey's declaration, on page 14, to the effect that major post-pluvial wet periods are unrepresented in the Albert basin is not in accordance with fact, nor, as a perusal of the publications of the geological survey of Uganda will show, is it true that, as Mr Solomon states (p. 263), the Gamblian pluvial 'is unrepresented in the Victoria and Albert basins owing to the presence of an outlet'. Deposits of this age have long been known in both basins, and they were dealt with by the present writer in a lecture to the Geologists' Association in 1929. It is to be noted, too, that the red beds of Gamble's cave, originally held by Leakey in his earlier writings to be silt, are now regarded as 'clear evidence of . . . aridity', but the reasons for this change of view are not stated. On page 263, Mr Solomon enumerates a succession of geological events bound up with the prehistory of the area, and he commences with a 'wet period during which the peneplanation of Uganda was completed and which coincided with the epoch of the man who made the pebble-tools (Kafuan)'. In point of fact there are three of these erosion-cut surfaces, and the last of them (Peneplain III) was, for all practical purposes, as advanced when Kafuan man made his first appearance as it is today.

While the pluvial sequences of Kenya and Uganda are found to agree in all essentials, there appear to be some differences between the contemporaneous cultures of these two areas.

Tools of Chellean and Acheulean facies are found in beds belonging to the decline of the first (Kamasian) pluvial.

A culture of uncertain age in Kenya is the Nanyukian, 'a very advanced development of the Kenya Acheulean with a very marked tendency to a very early Mousterian type'. The Nanyukian is presumed to belong to the dry period between the Kamasian, and Gamblian pluvials. In Uganda, however, a similar culture has been discovered in beds interpreted as mid-pluvial 2 (middle Gamblian) in date, while some 16 feet below it a 'digging stone' made from a lump of phyllite has been found *in situ*, suggesting the presence of Neanthropic man in early Gamblian times.

Leakey shows that in Kenya, industries of Mousterian and Aurignacian facies develop side by side throughout the Gamblian, and that so far as the evidence goes they are not intermixed, but owing to the restricted area as yet investigated, it cannot be maintained, as suggested on page 31, that this is a proved fact, except for the area in question.

To the decline of the Gamblian belong the Kenya late Aurignacian and an industry closely resembling the Still Bay culture; while in the Makalian wet phase (supposedly of Bühl times) we have the Elementeitan culture, a Mesolithic industry comprising long two-edged blades, backed-blades, microliths and lunates, scrapers, lames écaillées, burins (rare), bone tools and pottery. Leakey points out that there is a big hiatus between the Elementeitan and the Upper Kenya Aurignacian, but it would appear that this gap is more or less bridgeable both typologically and chronologically in Uganda by an industry known as the Magosian. It is very interesting to find that in Kenya, as in South Africa, certain 'points', somewhat reminiscent of the Solutrean 'laurel leaf', resulted from contact between flake-using folk, and those whose culture, so far as stone artifacts are concerned, was largely characterized by the blade and burin.

Succeeding the Kenya Wilton, which belongs to a stage in the decline of the Makalian, neolithic remains are found. In addition to three main types of tools, backed-blades, lunates and scrapers, the two industries so far discovered (Gumban A and B and the

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Njoran) are represented by beads, stone bowls, stone pestles and mortars, stone saucers, querns, pottery and some polished axes (Njoran). While referring to the Gumban deposits of Nakuru it may be mentioned that it is hardly right to say (p. 200) that Major Macdonald described the white powdery earth found therein as 'Fuller's earth'; he spoke of it as 'interstitial dust . . . of fine division as Fuller's earth' (p. 283), a much less committal statement. The potter's art, like that of the manufacture of microliths, is a very ancient one in East Africa, for it makes its first known appearance in the Upper Kenya Aurignacian, which culture, if the Glacio-pluvial correlation is correct, must have been in existence in early Riss times. One of the most surprising facts brought to light by Leakey is the early appearance of non-negro Neanthropic man. His remains have been recovered from Upper Aurignacian, Elementeitan and Neolithic (Gumban A and Gumban B) horizons; and Reck's discovery in the Oldoway gorge (Tanganyika Territory) of a similar type of man of Kamasian days is now confirmed by Leakey, contrary to his expectation as expressed on page 16.*

Were it not that space forbids, a good deal more might well be said about the problems solved and raised by this interesting book which, it may be added, is of convenient size, though perhaps a trifle heavy, and very well illustrated by photographs and drawings; the former are excellent, and no better guarantee of the accuracy and quality of the latter can be given than to remark that many of them are the work of Mrs M. C. Burkitt. The volume is not only one which none but the most parochial of prehistorians can afford to disregard; in addition it is an unpretentious record of a worthy enquiry initiated and inspired throughout, in spite of many difficulties, by that contagious enthusiasm and untiring energy which distinguishes its author. Two useful maps and an index complete the book.

E. J. WAYLAND.

DIE BAUART DER EINZELGRÄBER. Beitrag zur Kenntnis der älteren individuellen Grabhügelstrukturen in den Niederlanden. By Dr ALBERT EGGES VAN GIFFEN, Director of the 'Biologisch-Archaeologisch Instituut' of the University of Gröningen. Leipzig: J. A. Barth, Salomonstrasse, 18, 1930. Vol. I (text), pp. VIII, 208, with 3 illustrations; vol. II (plates), pp. VII, 119 plates, containing 156 detail plans and illustrations. (=Mannus-Bibliothek, edited by Prof. Dr GUSTAF KOSSINNA, no. 44 and 45). Cloth RM 42, for the series RM 36.

This work, which sets a very high standard for excavators and for the publication of results, is of paramount importance for all students who are interested in the early connexions between the British Isles and the Continent. Already when prehistory was in its infancy it had been realized that the Netherlands must have played an important role at that crucial period when the beaker people (or peoples) made their appearance on British soil. Since then evidence has been accumulating that the comparative poverty of Dutch museums in finds which did not originate from the well-known megaliths of northern type gave no true picture of what the country was likely to yield. Much new evidence is due to the activity of Dr J. H. Holwerda, the Director of the Leiden Museum. But in recent years also the northern parts turned out to be a very prolific field of research, thanks to the indefatigable work of Dr van Giffen, who has built up valuable collections in his Institute and in the museums at Gröningen and Assen, and who has indeed

* See however, p. 224.—EDITOR.

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gradually expanded his area as far south as the Veluwe, the sandy district south of the Zuyder Zee where Dutch prehistory probably has its most promising hunting-ground (as is shown by the collections in Leiden).

Dr Åberg's *Die Steinzeit in den Niederlanden* (Uppsala, 1916) became somewhat out-dated by the new activities: spade-work from which the knowledge of megalithic and non-megalithic civilizations in the Netherlands benefited likewise. Dr van Giffen's most lavishly illustrated work *De Hunebedden in Nederland* (Utrecht, 1927)¹ sums up what all previous evidence and his own careful investigations could extract from the still existing (and most deplorably ruined) remains of the northern megaliths, which are 'northern' not only as far as their distribution in the Netherlands goes but also northern in the sense that they are the western outpost of the Scandinavian and North German megalithic group.

If, therefore, it is unlikely that our knowledge of the Dutch megaliths will still be materially added to, it was the more desirable that we should get also Dr van Giffen's views about the non-megalithic groups which partly overlap with the megaliths in the same area (and to some extent even in time). It was to be expected that the mutual interrelationship would throw new light on a very dark period of a country whose archaeology is but little known abroad and whose very position made it an important stepping-stone for expansions from the continent, situated as it was on the border of powerful continental groups which tended towards the sea whilst this littoral country formed on the other hand a barrier between expansive maritime groups in the West and East.

Dr van Giffen's book, which is dedicated to Professor Kossinna (deceased recently), goes far to fulfil such expectations, although he will probably humbly deny that his 'contribution' forms the corollary to his treatment of the 'Hunebedden'. The subject of the new book is the Dutch 'separate graves' (as opposed to the megalithic collective burials²) of late neolithic, aeneolithic and—to some extent—early Bronze Age date which are to be found (on or under ground-level) in characteristic barrows and which belong to two different civilizations: the one to be derived from the 'Tiefstich'—(deeply incised) pottery of the Hunebedden-builders, the other belonging to the beaker civilization (or civilizations) proper and showing affinities to the other (western European) megalithic groups. Accordingly there is a marked difference in barrow construction: the barrows of the second class ('separate graves of the beaker culture') are characterized by wooden structures or once existing circular trenches, which are absent in the first class (the 'separate graves of the "Tiefstichkeramik"'). There can be no doubt that these differences reflect a stage at which the two parental civilizations had not yet entered into promiscuity and the older the barrow the easier we will discern the different parentage, whilst in the subsequent periods the individual traits fade and a general exchange seems to take place. The gradual progress of this movement is probably nothing else but the faint repetition of the age-long struggle between the cultural groups emanating from southwestern and from northern or central Europe.

From the outset the two classes largely overlap in time, albeit that class 1 seems to have left more survivals in the burial customs of the advanced Bronze Age (and even the Iron Age) and the question of survival in later burials bulks largely in Dr van Giffen's treatment.

¹ There is also an English edition of this elaborate work.

² Dr van Giffen uses also the term 'Massengräber' (mass burials) which is rather misleading.

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We are shown many typical representatives of both classes which again and again disclose the leading features, no matter how widely different the individual barrows may be. It requires indeed a highly skilled excavator to properly read these monumental palimpsests, the first of which was brought to our knowledge when the same author published the barrow of Harendermolen near Gröningen in 1924 (*Prähistorische Zeitschrift*, xv, 52 f.). The plates furnish ample evidence of the painstaking trouble taken in the excavations and the author occasionally defends himself against a possible criticism that he overdid it. We need not complain: in the existing literature we generally find that the funerary ritual receives less attention than dating finds, and careful observation may reveal more of the ritual itself—preserved by features of the soil—than is generally assumed. These rituals must always have been a highly 'conservative' element of the civilization to which a given sepulchre belongs, and the recording of such will add to the limited possibilities of Palaeoethnology. Dr van Giffen's remarks about his favourite technique of excavation will, therefore, be welcome to many readers. Another result is the observation that the Neolithic barrows consist invariably of sand whilst the Bronze Age barrows are built of sods, indicating a change of vegetation, *i.e.*, of climate, which took place at the transitional period. He tries to link it up with the present views about climatic changes.

Let us see now how the two classes of barrows (and their subdivisions) behave. Both classes comprise features of the collective and of the non-collective burials of their parental civilizations. As typical of class 1 the author selects the 'Doppelhügel' (double barrow; but what he wants to denote could better be styled 'composite barrow'), no. 11 on the Noordsche Veld near Zijen, Province of Drente, which lies close to a megalithic monument. The basal sand-barrow contained a packing of stones (degenerated stone cist), and, separated from but contemporary with it, a secondary burial of a female, belonging to the 'tiefstichkeramische' group. He interprets this as a faint remembrance of the megalithic collective burial custom; the burials are split up, so to speak, into individual graves which betray the influence of the Cimbrian 'separate graves'. This basal mound stood unaltered for some considerable time during which heather vegetation conquered the country and covered the surface. In the early Bronze Age a covering or 'cap'-barrow was built of sods on top of the older barrow. We are thus shown the transition from 'individual family burials' to the 'individual barrows' of the Bronze Age. Of the latter several instances are treated on pp. 27-35. They are rather frequent in the northern but obviously also in the more southern parts of the Netherlands; contain as a rule tree-trunk coffins in shaft-like grooves; and are practically destitute of grave-furniture (secondary interments of later periods are frequent). In shape they can be round or oval, but also elongated, indicating the different types of megaliths which exercised their influence. The writer of this review had the privilege of being present when Dr van Giffen excavated, near Weerdinge (Drente), one of these oblong barrows whose megalithic traditions were unmistakable. This heritage seems to have outlived even the early Iron Age if we are to believe Dr van Giffen's statement that certain burials of La Tène Age show traces of its survival. The monuments occur in the Netherlands and in adjacent parts of Germany and consist of very low sod-barrows (with central urn-burial) which were encircled by an open trench. The barrows form rather large cemeteries and vary in shape. As a rule the trenches are circular, but they may also be oval and even of key-hole shape. Whilst the trench (not to speak of the cremation) is an element foreign to the tradition with which we were concerned hitherto, the author makes a good case for his claim that the inconsistency of shape reflects the

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difference between round and oblong megaliths of the northern group. I confess I cannot follow Dr van Giffen in his further claim that the key-hole shaped trenches are influenced by the gallery-dolmens and similar western megaliths. The feature also can be explained by the desire to have a little ante-room separated from the surroundings and the connecting links are totally absent both in space and time.

Class II, the separate burials connected with beaker pottery, forms the main part of the book (pp. 37-166) and the resemblances which the author shows to exist between these barrows and British monuments give an additional interest to the mass of detailed facts. The class is by no means homogeneous, but the presence of wooden constructions in the barrows or of trenches round them allows of a clear-cut separation and further sub-division: (1) palisade-barrows, (2) beehive- and hut-burials, (3) the contested burial-chambers of wood with corbelled wooden roof, or 'Kuppelgräber'. They contained skeletons in crouched position (fairly soon replaced by cremation rite) and are richer in grave-furniture than the barrows of the first class; and they cluster in the Dutch Midlands, principally in the region south of the Zuyder Zee. Heterogeneous as they are in the beginning ('individualized' collective burials or 'separate graves' connected with the beaker population) they in turn develop on similar lines like class I and tend to amalgamate, leaving many traces in subsequent Bronze Age barrows.

A dozen instructive barrows of the first sub-group ('palisade barrows') are dealt with on pp. 40-67, the typical one being the Harendermolen tumulus, referred to above, a short summary of whose disclosures is given. It is a bell-shaped 'double' (read: composite) barrow which shows three subsequent stages of construction and interment: (a) a basal sand barrow of latest neolithic date with a crouched skeleton and grave-furniture of the bell-beaker civilization; to this stage of the barrow belonged a double ring of strong oaken palisades, the circles measuring 8 and 10.5 meters in diameter; (b) a covering sod-built mound with cremation burial of the middle Bronze Age; (c) urn burials, sunk in the composite mound and dating from the end of the Bronze Age (700 or 600 B.C.). The similarity with, and the differences from, the Zijen tumulus are very striking, the most surprising novelty being the palisading. If the general shape of the barrow resembles already the trenched bell- and disc-shaped barrows of Britain, the double circle of standing poles seems to link up our Dutch barrow (and its derivatives like the early Bronze Age barrow near Weper in Friesland, see van Giffen, p. 72) still more closely with such conspicuous monuments of the British megalithic civilization as Stonehenge and Woodhenge. (Incidentally Dr van Giffen uses the latter names in the plural form as class terms, *e.g.*, speaking of 'Woodhenges' to denote timber circles, which is open to objections). Whilst he is certainly right in recognizing a family resemblance in these circular structures of uprights, attention must be drawn to the fact that Woodhenge is not, as Dr van Giffen thinks, a barrow and that the four smaller circles nearby, though probably once covered with barrows, had no circular palisades of the Harendermolen type. But Dr van Giffen is not to be blamed for his mistake as he based his conclusions on the article in *ANTIQUITY*, March 1927, whilst Mrs M. E. Cunningham's book, in which the true nature of the enclosed area is told, appeared only in 1929. It is another question whether the child's grave in the centre of Woodhenge is sufficient reason to call the monument a sepulchre; but even if it (and Stonehenge) be only ceremonial, there are plenty of lesser stone circles of sepulchral character; and the similarity to the Dutch palisade barrow (and to Rhenish monuments like the well-known tumulus of Baierseich near Darmstadt) is too striking to be accidental.

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We have learned thus that the Dutch barrows which form the subject of the book are not only related to the megalithic civilization of the North but also to its western counterpart and we may expect similar analogies to be found elsewhere in the Rhenish corridor, as indeed they are.

A very instructive barrow is the tumulus with double trench and crouched skeleton, excavated near Odoorn (Drente), which belongs to van Giffen's sub-group 3 of class II (2 and 3 are the beehive-, hut- and other burials which are characterized by wooden constructions other than palisades and which are to be explained by the house-burial conception). This tumulus yielded, amongst other grave-furniture, a tanged knife-dagger of western type and two thin gold appliques of a recognized Breton type, belonging to the very period when Brittany herself was strongly influenced by the northern civilization.

As to the general classification of the finds associated with barrows of class II we can distinguish a middle group (Veluwe, &c.) characterized by typical bell-beakers or zoned beakers, large semicylindrical bracers and buttons with v-shaped perforation, and a northern group in which the bracers are flatter and made of schist, whilst the beakers as a rule show ornamentation by cord-patterns or oblique lines. This tallies to some extent with Dr Åberg's previous classification, and we cannot help being reminded of the 'dual character of the beaker invasion' put forward in Mr J. G. D. Clark's admirable survey in *ANTIQUITY* (December 1931).¹ But the two English waves cannot yet be brought into line with the Dutch groups; and we must await further progress of excavation work in the Netherlands which will undoubtedly result in a still more comprehensive picture of the different groups and successive waves of the lower Rhenish beaker civilization.

There are very interesting sections in the book dealing with the general development which resulted in the Dutch beaker period (see also plate 119). Dr van Giffen believes that the megaliths of northwestern Europe and Denmark had a common focus in what is now part of the North Sea and that in Blytt-Sernander's 'sub-boreal' period the builders of the separate graves and the corded beaker civilization began to move from the interior down the Elbe whilst the bell-beaker people moved down the Rhine, thus completing the then already geographically existing cleavage between the two megalithic groups. In doing so they opened the way to Central Europe, for the influences from the European west and southwest, reflected, *inter alia*, in the Dutch timber circles of the Woodhenge-Harendermolen type. British tin, Irish and Breton gold, in exchange for amber, were the main stimuli for trade relations and insular influence upon the Continent. At the same time the English Channel area is said to have been subject to a new transgression, opening a new route for the West-East trade which abandoned the older routes around the British West. This phenomenon is explained as part of the great climatic change which is testified by the heather vegetation of the sods constituting the Dutch barrows of the fully developed Bronze Age.

This is rather a long review but I want English readers who may find it difficult to follow the reasoning in the general chapters to benefit more from the many detailed accounts and most instructive plates of a book which is equally important both for regional and comparative prehistory.

ADOLF MAHR.

¹ Since the above was written the same problem has been touched upon by several writers. See: Miss M. Mitchell in *ANTIQUITY*, March 1932, 90-91, and reply by Mr J. G. D. Clark, *ibid.* 91-93; Prof. V. G. Childe in *MAN*, 1932, no. 102; Mr W. F. Grimes in *Proc. of the Prehist. Soc. of East Anglia*, 1931, vi, 340 f.

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BUSHMAN ART. By HUGO OBERMAIER and HERBERT KUHN. *Oxford: Humphrey Milford, 1930. pp. xxii, 64, with 39 plates and 10 text figures. 84s.*

Within the last few years the standard works on Bushman paintings and engravings have been supplemented by a number of publications. The most noteworthy of these are Miss Bleek's book on Stow's material, and the truly sweeping series issued under the guidance of Dr Frobenius. But until the appearance of the work under review we have had no representative material from southwest Africa. Copies of some of these paintings had been housed in the South African Museum, and were relatively accessible to visitors to Cape Town, but with publication the paintings and engravings have become infinitely more useful as a standard of comparison between prehistoric art in this field and elsewhere. The material was collected by Mr Reinhard Maack, during his stay in southwest Africa, and he was approached by Professor Obermaier, who was generously allowed the full rights of publication. The relatively high cost of the work is adequately reflected in the beautiful manner in which it is produced. Many of the illustrations are actual photographs, and all have been separately printed and attached to their pages, but a cheaper, if less sumptuous, edition would be welcomed by the South African book trade.

The most striking fact in studying the material here presented lies in the extreme peculiarity of many of the examples. As a result the comparisons drawn by the writers between the southwest African examples and the eastern Spanish art are weak. The area reviewed, save for the crude human figures, shows an advanced and specialized art and many gaps will have to be filled before we can safely link up this material with a district even so near as southern Rhodesia. As an instance, the first three coloured plates present types of cave-art unknown elsewhere. The finicking detail of the white beadwork shown on the dull red figures of the first two recalls vaguely certain drawings known from the Kei river in the Eastern Province of the Cape. More convincing is the similarity between the art of these two areas in some of the single animal figures.

The extraordinary modernity of design and execution seen in plate 10 is unparalleled in any cave-art so far known. The calm, conventional figures of equids from Gross Spitzkopje show an impressionism surpassing in grace much of our modern European work. It is amazing to think that the purely artistic side of Bushman painting has so far evoked no more than the single chapter of Roger Fry from our chief artistic authorities. West Africa has produced a phase of European art; can we not turn for colour and life to prehistoric Africa and Spain?

A. J. H. GOODWIN.

EXCAVATIONS AT OLYNTHUS. By DAVID M. ROBINSON. Part II, Architecture and Sculpture. 1930. pp. xxii, 155; 307 photographic plates, 3 coloured plates, survey. 90s. Part III, Coins. 1931. pp. xiv, 129; 28 plates, 2 figs. 45s. Part IV, Terra-cottas. 1931. pp. xii, 105; 63 plates. 45s. *Baltimore: The Johns Hopkins Press.*

Dr Robinson has lost no time in publishing his excavations at Olynthus, and there is here collected an immense amount of important material. The site is identified beyond reasonable doubt, and it seems probable that it was abandoned immediately or soon after Philip's destruction of the city in 348 B.C. But we must quarrel with its excavator at once, for there is no sign that he attempted to stratify levels or to discover the dates of its buildings by careful digging. This is particularly noticeable in the confused account of the Entrance Road. Part II contains only one section-drawing,

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the step into the court of a house ; and the conglomerate or ' ground ' is only mentioned where the surface soil was too shallow for substantial remains or where pits and pithoi were sunk in it. Nor were the traces of fire examined carefully, which are mentioned sporadically with individual finds ; and no record is made of burnt brick. The argument for dating goes in a vicious circle : in part II the finds give 348 B.C. as the *terminus ante quem* ; in III and IV the excavation dates the finds. But the problem is complicated by the discovery of coins, terra-cotta busts and lamps, black-stamped ware, statuettes of Leda with the swan, Silenus, and genre subjects, all of which are usually dated Hellenistic and are now claimed as Hellenic. It is enough to quote twice : ' Grose and Head date similar coins [of Hephaestia in Lemnos] about 300 B.C., but in view of the fact that we discovered nothing which can be definitely dated later than 348 B.C., I am inclined to date such coins before 348 B.C., as the coins with it are also before 348 B.C.' (IV, 93). ' The statement has been made repeatedly that realistic or comic figures belong to Hellenistic and not Hellenic times. But our excavations have shown that we must revise our chronology, and that such types were already known in the time of Philip or perhaps even earlier. The circumstances of finding and the pottery and coins found with the following terra-cottas make this certain. Furthermore most of them were buried in the ashes which came from the conflagration of August, 348 B.C.' (IV, 79). The reader must decide for himself if the head of Pan (IV, no. 386) can be dated to the early fourth century by its provenance (trench IV) and the objects found with it, or if in trench IV sherds ranging from Neolithic to black-stamped ware were found without any stratification (II, 5), or if anything in the three volumes can be dated by its Olynthian context.

But some points are clear. The South Hill was occupied in Neolithic times and again from the Iron Age onwards. Among the confused remains two houses are conspicuous in plan, built back to back, rectangular, double-roomed (II, figs. 38, 40). At the northern end of the hill, the ' Civic Centre ', were a long rectangular altar and a pillared hall of uncertain plan. Several stucco fragments of anta capitals, pieces of applied bead-and-reel and egg-and-tongue (called egg-and-dart, and profile not shown) were found near by, which probably date from an earlier building whose existence is suggested by cuttings in the rock and reused blocks.

On the North Hill three areas were examined ; two of them on eastern spurs showed signs of rebuilding, but along the west side the houses were planned and built late in the history of the site. Here a street runs north and south, 5 m. wide, with regular houses of a frontage of about 17 m. Cross streets run at right angles to the east, making a block front of two houses ; and the 1931 excavations have confirmed the Hippodamian plan and laid bare a complete block. (*Illustrated London News*, 23 Jan. 1932). The houses, though not uniform, may be generalized as follows : a long passage leads directly from the road to an open court, on to which the rooms look from the north. The rooms are separate from each other ; and the principal room is nearly square, having a low and broad border round a square of mosaic or cement from which a drain carries off the water. We cannot speak definitely of an impluvium, for there is no evidence for the roof, and incidentally none for the second story which is postulated, and remarkably few tiles were found. The megaron plan of a room facing south on to the entrance passage is found in houses 2 and 3 (*cf.* Priene, house xxvi) ; and in house 3 there is a peristyle, one side of it continuing a narrow entrance from the west.

Enough remains from the houses to throw an ironical light on Demosthenes' eulogy of the great Athenians' simplicity (Ol. III, 25). The pebble mosaics are

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magnificent; the sculptures are few but good. Among the many terra-cottas a mould for a figure of Cybele and fragments of broken plaques of the Athena Parthenos may be mentioned. The lamps are discussed at the end of part II, and Miss Wilson has added a chapter on the loom-weights. 1187 coins are catalogued in part III. Among them are 67 bronzes of the Bottiaean, probably contemporary with issues of the Chalcidic League; 47 are fourth century issues of Potidaea, obverse Athena in Corinthian helmet, reverse trident or bull or winged Pegasus, from which Dr Robinson deduces a rebuilding of the city by the Corinthians.

The panoramas in part II familiarize every corner of the site; but the publication is overweighted with photographs, many of them bad, especially in III and IV, and many in II unnecessary. The spear and arrowheads, one inscribed with Philip's name (II, xi and 25) and some of the loom-weight stamps might have had some illustration, *e.g.*, no. 27, a foreshortened figure, and no. 16, a kneeling archer, quoted as parallel to a terra-cotta relief (IV, 340). The cataloguing and the indexing are thorough, except for the omission in II of necessary references to trenches 7, 8, 13, 14, and to the few metal finds. One knife blade is the only tool recorded (II, 102). Part II is tedious reading owing to its clumsy arrangement of photographs, and the survey at the end would be more useful if the trenches were numbered in it. Two of the plans have no scale or compass points, and the illustrations have no scales. Yet the text is full of unnecessary measurements, and is spoilt by inaccuracies and hasty writing; 'skillful' slips in (II, 126), but 'Terone' and 'Teronaic' are intentional for Torone in IV. The publication is prompt, well-referenced and intended for the non-specialist reader; but the material is too important and interesting to need pretentious description. G. A. D. TAIT.

GREEK CITIES IN ITALY AND SICILY. By DAVID RANDALL-MACIVER.
Oxford: Humphrey Milford, 1931. pp. 266, and 22 plates, 2 maps and 3 plans.
12s 6d.

Dr Randall-MacIver's name on the title page is a guarantee of the scholarly excellence of a book, and this volume on the Greek cities of Italy and Sicily fulfils a double purpose. From the archaeological point of view it supplements the classical work of Lenormant in Italy, bringing it completely up to date, while it presents much new material, partly due to the work of Professor Orsi, on the Sicilian cities. A brief history is given of every town, and in the case of Syracuse this is expanded into two excellent chapters. Writing in a style worthy of the subject, Dr Randall-MacIver has avoided pedantry, but has supplied references to larger works in footnotes, thus producing a volume of great attractiveness for the general reader who may be neither an antiquary nor a traveller.

In the second place, the author's personal knowledge of the sites, and the useful information he gives as to the best method by which they may be reached at the present day, make the book an invaluable guide to tourists or archaeologists visiting Magna Graecia. The illustrations are beautiful and well reproduced.

DINA PORTWAY DOBSON.

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[VOL. XVII, No. 66

HISTORY

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LONDON: MACMILLAN & CO., LIMITED

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Antiquity

A Quarterly Review of Archaeology

VOL. VI No. 23

SEPTEMBER 1932

Editorial Notes

THE outstanding archaeological event of a decade rich in surprises has been the discovery of the Indus Civilization and the publication of Sir John Marshall's account* thereof. Now an unknown civilization, if it turn up far away and isolated from more familiar ground, may leave the European archaeologist cold ; we are naturally preoccupied with our own continent and the adjacent zones of North Africa and Hither Asia from which Europe received enlightenment. No such aloofness is possible in the case of Mohenjo-daro and the civilization it represents. A whole series of links, from precious stones of Indian origin down to imports of actual Indian manufactures, establishes beyond all shadow of doubt the intimate connexion between the valleys of the Indus and of the Tigris-Euphrates round about 2500 B.C. And right on the frontiers of Europe two highly specialized types of gold bead from the 'treasures' of Troy demonstrate the westward extension of the same nexus.



These discoveries are revolutionary : they demand a complete reorientation of our attitude towards the birthplace of civilization. The rôles of Egypt and Babylonia have been familiar from ancient tradition. Romantic faith in the legends of Greece led Schliemann

* *Mohenjo-daro and the Indus Civilization*, 1931, 2 vols. £12 12s. (Arthur Probsthain, 41 Great Russell Street, W.C. 1).

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to Troy and Mycenae. Strictly scientific induction guided a man of genius to Knossos, where by a stroke of the spade he was able to set the products of Schliemann's digging in their proper context. But in India the by-products of railway building catching the enlightened eyes of Sir John Marshall revealed a civilization that had lain absolutely forgotten for 4000 years.



This discovery has enlarged the domain of scientific prehistory, the area for which we may hope to establish a reasonably coherent system of knowledge, by some 750,000 square miles. In seeking the cradle of civilized life we are no longer restricted to Breasted's 'Fertile Crescent' balancing the relative claims of Egypt and Sumer. The Nile indeed is found to be on the western edge of a vast constellation of centres of urban life.



The intercourse between India and Sumer now established opens up a vista of caravans traversing the intervening deserts and crossing the mountain barriers, range after range, and of ships ploughing the waters of the Erythraean Sea 4500 years ago. That may help to banish the uneasy scepticism the more imaginative of us must feel when we read of Merchant Venturers in Bronze pushing up the Danube valley, or Ancient Mariners crossing the Bay of Biscay and rounding Cape Wrath leaving a litter of megalithic tombs behind them.

Again, the intercourse newly revealed was between highly differentiated and individualized civilizations. Mohenjo-daro is stamped with distinctively Indian idiosyncrasies, just as surely as pre-Sargonic Kish and the prehistoric graves of Ur are emphatically Sumerian and Old Kingdom Egypt is unmistakably Nilotic. Art and Religion, metallurgy and jewelry, are already completely specialized and exhibit arbitrary peculiarities which have persisted for millennia, some even to our own day.



But that circumstance demands an enlargement of our chronological horizon no less drastic than in our spatial outlook; the revolution is not confined to Euclidean space but invades the new space-time. For, underlying the very real differences between the three great civilizations, and transcending the superficial agreements resulting from mutual

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intercourse, are fundamental uniformities so deep-seated, so comprehensive and so numerous, as to preclude the idea of independent origin in three areas later so closely interrelated. And indeed hints of connexion, vastly earlier than those hitherto considered, are not wanting. Lapis lazuli derived from Afghanistan is found in early Predynastic graves in Egypt ; a bead of amazonite from the Nilghary Hills of India was dug up from a prediluvian layer at Ur. And from hundreds of mounds extending westward from the Indus through Waziristan, Baluchistan and Seistan Sir Aurel Stein has collected sherds of painted pottery, fragments of stone vases, female figurines, stamp seals and beads of lapis or hard stone that must somehow link on with the similar material turning up at the base of every ancient site in Assyria, Babylonia and Elam. Whatever conclusions a scientific examination of the mounds and sherds may eventually justify, the scraps of evidence available suggest a continuum of less highly specialized cultures from which the Indian, the Sumerian and presumably even the Egyptian eventually crystallized out. But the process of differentiation, complete before 2500 B.C., must have taken very many centuries. And yet all these shadowy cultures emerging from this hoary background already possessed copper !

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Here is a field for speculation. It might more profitably be regarded as a field for work. The province newly attached to the system of European-Oriental prehistory lies wholly within the British Empire. The conservation and proper examination of documents so vitally significant for human history is entrusted in an unique degree to us British and to our Indian fellow-subjects. A magnificent beginning has indeed been made ; thanks to Sir John Marshall, Mohenjo-daro has been excavated in a fitting manner, and, what is still more creditable, the results up to date have been published with a promptitude almost unprecedented in the annals of Oriental research. Sir Aurel Stein has undertaken a thorough survey of a vast and most inhospitable territory and has published, again with commendable rapidity, two richly illustrated reports on the epoch-making results of his tours. But this is only a beginning ; the prehistoric riches of India have only been scratched ; the results raise more problems than they solve.

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It is an obvious duty for every citizen of the Empire to insist on the importance of preserving these sites against spoliation whether by

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road-contractors or relic-hunters. To expect and demand much Governmental support for excavation in the present economico-political situation is manifestly vain. And those who have the knowledge, the time or the money must be ready to assist in the work of conservation, excavation and examination. What an opportunity these *tells* offer English capital and English science for probing back towards the beginnings of human culture! What a scientific career for anyone with the vision—and the knowledge!



Writing in the *Revue Archaeologique* (July-October 1931, 211) Dr Salomon Reinach, one of the editors, says:—‘It is not enough, apparently, that the world should contain ten times more journals of art than it needs, and that new ones should be incessantly cast upon it. Now there is added a swarm of “Museum Bulletins” so numerous that it has become quite impossible to know of their existence and profit by them. For a long time past I have been appealing for a congress of librarians to discuss a remedy for this overproduction by putting on the black list superfluous publications, but it is the voice of one crying in the wilderness’.



We sympathize with the learned doctor, but not with his proposed remedy, which would in any case be quite ineffectual. The business of librarians (and, we might add, of learned societies) is to canalize, not dam, the waters of learning. We need more bibliographies. In England we have the nucleus of a bibliography of British archaeology in Sir Laurence Gomme’s *Index of Archaeological Papers, 1665–1907* and its supplements for 1908 and 1909. To have allowed this splendid work to be discontinued was a crime. In its published form the Index is difficult to use, but to transcribe its contents on cards (omitting a certain amount of rubbish) and rearrange them under subject-headings is not a superhuman task, and has been partially accomplished by the present writer. To complete it down to 1931 would be a formidable undertaking, but one not beyond the powers of the various archaeological societies devoted to the advancement of knowledge if each would become responsible for its own publications and card-index them.

Two Greek Fortresses in Sicily

by R. G. COLLINGWOOD

THE two fortifications which I propose to describe are together unique in the history of military architecture. They are very like each other, but nothing else like them exists. The first of them, Euryalus at Syracuse, has been carefully planned and published by a professional engineer, Signore Luigi Mauceri, in an excellent monograph (*Il Castello Eurialo*, Roma, 1928); but this is difficult to procure—I only know of it, and have seen it, thanks to the kindness of Sig. Dott. Giacomo Caputo, of the Syracuse museum—and there is no other adequate description of the place, though no doubt Pauly-Wissowa's article on Syracuse will contain one when it appears. Moreover, in examining the remains I seemed to see evidences of afterthoughts and changes of plan which Sig. Mauceri has missed or interpreted in ways that I find hard to accept; and therefore, though my hasty inspection and rough plans are far inferior to his careful and skilful work, it may be worth while to place my surmises on record.

The other fortress, Selinus, has been most elaborately and beautifully published by Messieurs Hulot and Fougères (*Selinonte, la ville, l'acropole et les temples*, folio, Paris, 1910) whose results have been used in Pauly-Wissowa's article *Selinous*; but here again there is no description, I think, in English, and therefore as regards both sites an *oeuvre de vulgarisation* may be welcome to English readers. That is the chief aim of this article, whose only novelty, apart from the suggestions, mentioned above, concerning changes of plan at Euryalus, lies in the fact that the two fortifications are discussed together, with perhaps some improved understanding of both.

The plans which I give here are only sketch-plans from my own measurements, and do not profess to satisfy the demands of scientific accuracy, for which the reader must go to the works quoted above. I ought perhaps to warn him—it is an unpleasant duty, but at least it shows how little acquainted English readers are with these sites—that the photograph of Euryalus given in the *Cambridge Ancient History*, plates, vol. II, p. 12 (b), does not, as there alleged, show 'the remains

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of rooms and a wide passage that was once underground'; it shows the innermost ditch, seen from the north, with the wall that blocks its north end in the foreground, and the piers of the drawbridge in the background.

I. EURYALUS

The original Greek colony of Syracuse, confined as it was to the island of Ortygia, was reasonably safe from attack by land. But the island was only a mile long by at most half a mile wide, and the town was soon compelled to spread to the mainland, and creep up the hill that slopes downwards to the island and the Great Harbour. This hill is a triangular plateau, nearly four miles long from east to west, and about two from north to south; Ortygia lies off its southern corner. The edges of the triangle, except opposite the island itself, are formed by continuous lines of crag, hardly deserving the name of precipice, but mostly difficult of access; its base is protected by the sea; but its apex, on the west, runs out into a long ridge, narrow and steep-sided, but flat-topped.

This ridge is the tactical key to Syracuse. When you have gained its summit, the whole plateau of Epipolae stretches away in a smooth and gentle down-slope before you; an army brought up by the ridge and deployed on the plateau can sweep eastward, unchecked by any natural obstacle, until it has overrun the whole triangle. On the other hand, if this ridge is held, there is no other way by which an army can gain the heights without grave difficulty—difficulty which in the face of opposition would become impossibility.

Euryalus, the 'broad nail', is the name of the triangle's apex, the point where people coming along the ridge arrive at the plateau. Granted, then, that Syracuse has overflowed its island and spread to the mainland, any scientific scheme for its defence must be focussed on Euryalus. Elsewhere, the defence may be perfunctory; here, it must be as strong as the skill of military engineers can make it.

The ridge at Euryalus is only about 50 metres broad; the engineers' problem is therefore a very simple one—to block this narrow ridge, where the ground is so constituted that a turning movement on the part of assailants is practically impossible. The fort whose remains still crown the ridge is based on a plan as simple as the problem: the plan of a roughly rectangular *enceinte* equal in width to the ridge itself and therefore entirely blocking it, defended on the west by a deep ditch.

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But this plan has been modified by adding complications of a very remarkable kind.

In the first place, the western front of the fort is not, like its other fronts, a plain wall 2·0 to 2·6 metres thick ; it is no less than 6·6 metres thick, and is composed of five solid rectangular towers, with lower curtains between them. Secondly, instead of one ditch there are three : the first or innermost 10-15 metres wide and about 10 deep, with smooth vertical rock-cut sides and a flat bottom ; the second like it, but 20 metres wide ; the third a slighter obstacle, 100 metres away to the west and not included in the plan here printed. Thirdly, there are external structures in advance of the five towers : these consist of two hornworks, one on the berm of the innermost ditch, the other on the ravelin that separates the first ditch from the second. And fourthly, there is a most elaborate system of subterranean rock-cut galleries. The main gallery runs along the east side of the innermost ditch, level with its bottom and communicating with it by eleven openings ; from this gallery, winding passages lead into the fort, up to the inner hornwork, up to a point outside the fort on its north side, and out to a still more distant point north of the fort. These last two are designed to help in the defence of a lower shelf of land, running along the north side of the Euryalus ridge, and carrying a road which passed through the walls of Epipolae by a skilfully-designed triple gateway, foreshadowing in its plan the courtyard gates of the fourth century. Other galleries, again, lead from the bottom of the innermost ditch up to the outer hornwork, and into passages beneath it.

These elements combine into a whole which may be described, ignoring the outlying western ditch, as a range of towers protected by two successive outworks each composed of a bluntly pointed hornwork and a vertical-sided ditch. The outer hornwork is an extremely massive structure in solid masonry, 50 metres in width from north to south and about 25 in depth from east to west. Its forward or western edges run exactly along the edge of its ditch, which, like itself, is bluntly pointed in plan ; there is no danger of the masonry crushing the edge of the ditch by its weight, for the whole ditch is cut in solid rock. The inner hornwork is similar in shape, but its ditch does not follow it, being pushed forward at its southern end. This irregularity of planning is clearly connected with a wall, having chambers attached to its north side, that runs forward from the southern end of the range of towers and seems to have thrust the southern end of the ditch out of its natural course. This wall, however, does not terminate in any very convincing

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way ; it peters out on approaching the ditch, and has, moreover, been partially though not completely demolished, close to the towers, when the foundations of the hornwork were laid across its line.

This compels us to recognize that Euryalus, as we see it, was not planned all at once as a single whole. There were in fact at least two plans ; the second was begun before the first had been finished ; and, as we shall see, the second also was left incomplete.

Of the original plan (shown in black), so little was carried out that it is not easy to see at first sight what its designers intended. Plainly it included the north, east, and south walls of the fort as we have them ; the south wall, however, was to run on, with a very slight southward curve, following the curve of the crags, for some 30 metres beyond the southwest corner of the actual fort ; and the fort was presumably meant to terminate along the inner edge of the innermost ditch. The main gallery along the bottom of this ditch doubtless belongs to this plan, and so do the various tunnels leading eastward from it, together with the rock-cut ditch on the south of the fort, from whose bottom one tunnel (its roof is now collapsed) leads up into the fort and another down into the main gallery. The main ditch was meant, I think, to be cut right through the ridge ; on the north this has been done, on the south it has not ; yet one can see where, on this side, the digging of the ditch has been begun. A wall pierced by a postern has been built across the north end of the ditch ; had it been completed, one would suppose a similar wall at the other end. The piers of a drawbridge, still standing in the ditch, must belong to this stage of the plan ; there was to be a gate into the fort at its southwestern angle, leading to this bridge. Gate and bridge were thrust forward to serve as a sally-port for counter-attacking assailants on their defenceless right flank ; the ditch was laid out so that its bottom could be enfiladed from the gate-tower.

Before the construction of the west front was begun, the plan was altered. The five towers were erected ; we can still see that they are later than the north and south walls of the fort, for they are not of one build with them, and the southernmost tower is carried right across the line of the wall. These towers are not at right angles to the north and south walls, neither are they in line with the inner ditch ; but they are more or less in line with the inner hornwork just in front of them, and with the outer hornwork, and with the ditch in front of that. These, therefore, are the main elements of the second plan. We have already seen that the south wall of the first plan, in addition to being oversailed by the towers, was demolished to build the inner hornwork ;

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the outer hornwork is plainly of one design with the ditch in front of it, but the ditch behind it cuts awkwardly across the whole plan of these outworks, which have had to accommodate themselves to it as best they could. Moreover, as confirming the idea that the two ditches belong to two different plans, it may be mentioned that the rock-cut tunnels leading west from the inner ditch are much neater in design and execution than those leading east; the latter belong to the first plan, the former to the second, which is everywhere distinguished by the neatness of its workmanship.

But even the second plan was left unfinished. The hornworks were never built to their full height; it would seem that they never rose above their foundations. The outer ditch was never completely dug; about a quarter of it, at its south end, has only been cut a few feet deep, and the rest is much encumbered by stone that has never been cleared out. This stone has been described as material fallen from the outwork on the ravelin; it is really one of the many signs that neither ditch nor outwork was completed. Again, of the tunnels that lead westward from the inner ditch-bottom, only one has been finished, namely that which leads up to the ravelin; the others, doubtless intended to reach the bottom of the outer ditch, stop dead in the rock. And finally, the range of towers has been altered in a significant way. Originally these towers, except the north and south ones, were isolated structures with passages between, their tops being reached by stairs on the east. The intervals have been filled with walls not only independent of the towers but built of a differently coloured stone, that is to say, inserted at a different stage in the history of the work.

In order to understand the second plan, therefore, we must first of all open the intervals between the towers. Sig. Mauceri, having done this, proposes to fill them again with wooden gates; but there is no indication that such gates ever existed, nor would they have served any useful purpose; for enemy access to the line of the towers would be blocked by the wall of the hornwork in front of them. There is no indication that this hornwork was to be pierced by a gate; it seems to have been intended as an unbroken line: and therefore the draw-bridge over the inner ditch must be referred to the first plan, and was not required by the second. The defenders had access to the inner hornwork by the gaps between the towers; thence they had a tunnel to the main gallery in the ditch below; and thence again other tunnels would lead to the second hornwork and to the bottom of the outer

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ditch, along whose eastern side a second gallery, with entrances to the ditch-bottom, was no doubt to run.

All visitors to Euryalus are struck by its resemblance to the classical fortifications of Vauban and his followers from the 17th to the 19th century. The angular hornworks, the ditches with vertical scarp and counterscarp, the covered way, seem to lead one right away from the ancient world and into the conditions of modern warfare. But, although there is a good reason for this impression, the parallel must not be over-emphasized.

Modern fortification depends on the principle that every part of the defensive system is a field of fire, in which nothing shelters an assailant from the missiles of the defence. The angular lay-out of the lines is designed to enfilade attacking parties, and the ditches are so planned that they provide no cover. But, at Euryalus, the ditches are completely 'dead' ground from the walls of the fort; no one inside them need fear missiles from the fort itself. In the case of the outer ditch, this defect is remedied by the fact that the hornwork comes right up to its lip, so that men on the hornwork have the ditch-bottom right underneath them; but the inner ditch and the south ditch offer perfectly good cover from fire. On the other hand, they are commanded by galleries in such a way that hidden defenders can rush out and attack with the *arme blanche* anyone who has penetrated into them. This is the main principle of the ditch-system of Euryalus; assailants reaching the bottom of the ditch are to be dealt with, not by archery or other fire, but by counter-attacks from the galleries.

At first sight this may seem strange. The existence of galleries that could be forced by assailants, or secretly occupied by them through neglect on the part of the garrison, might seem to weaken the defence. Why not leave the sheer thirty-foot rock-wall of the scarp to defend itself?

The answer is that Euryalus was evidently meant to resist, not simply infantry assault, but siege-tactics. A ditch can be filled up, or siege-engines can be erected inside it; but these things cannot be done if the ditch-bottom is exposed to counter-attack by defenders able to burn the engines or demolish the filling. This seems to be the main principle on which the original plan was based. But the second plan introduces an innovation, in the shape of five solid masonry towers, each 4.2 metres wide by 6.6 deep. (Plate 1). What are these? Leake long ago suggested that 'balistic or catapultic engines might be mounted upon the walls between the towers, like cannon in embrasures' (*Notes*

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on *Syracuse*, 1848, p. 17). He was right in the main, but he did not notice that the walls between the towers were subsequent additions to the towers themselves. The catapults were mounted on the towers ; and thus the second plan introduced an artillery defence, firing over the heads of the men in the hornworks at enemies, or their engines, beyond the line of the outer ditch. (Plate II).

It is time to relate these tactical ideas to the history of Syracuse. The fort of Euryalus did not exist in 414-413, during the Athenian siege. The Athenians approached the plateau of Epipolae by way of Euryalus, which therefore they clearly found unfortified. Gylippus came by the same road when he relieved Syracuse, so that even the Athenians, who had used it, did not fortify it against use by others. And Demosthenes in his last desperate night attack on Epipolae used it once more, storming the small fort—certainly not the present one—that had been built there in the meantime.

It was quite ten years later, apparently about 402, that Dionysius I built his wall right round Epipolae ; and Euryalus is either part and parcel of that wall or subsequent to it. The evidence of the structures, as now visible, makes it practically certain that what I have called the first plan was part and parcel of Dionysius's wall, and the second an afterthought added to the same great scheme of fortification. This gives us 402, or thereabouts, as the date of the original plan ; one ought to allow a few years for the construction of so elaborate a work, although the wall round Epipolae (a very simple earth-filled structure, whose stones are irregular in size and not carefully dressed) was built in great haste and finished within a very short time. The second plan must date within a few years of 402.

During these years the art of war as known to the Greeks was changing rapidly. Siege-engines were being improved, and the defence, which a generation ago had been stronger than the attack, was becoming weaker. The Spartans in the siege of Plataea, in 429, used battering-rams and tried to mine the walls ; but the Plataeans easily put their rams out of action and frustrated their sappers' work. The Athenians, at Syracuse, made no attempt to assault the town ; they contented themselves with the weapon of blockade, and used siege-engines (and that unsuccessfully) only as a last resort, against the Syracusan counter-wall. But the Carthaginians by 409 were using movable towers, as well as rams, against the Greek towns in Sicily, and covering their approach by a barrage of archery and sling-fire directed against the defenders on the wall. This method of assault worked with deadly

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effect ; and if Dionysius was to save Syracuse from the attack which, sooner or later, Carthage was sure to launch, he must invent a corresponding method of defence.

He seems to have reached his invention in three steps. First, he decided to fling a wall right round Epipolae, using the crags as a natural obstacle to prevent the approach of rams and towers. It is a mistake to suppose, as some historians have done, that the whole area of this great *enceinte* was necessarily built over with houses ; the line was chosen as a measure of defence, not as a measure of town-planning.

Secondly, this wall was vulnerable to the Carthaginian engines at one point, and only one—Euryalus. Here, therefore, he planned a fort whose essential feature was a deep and wide ditch, a perfect obstacle to these engines, its bottom exposed to sudden and secret counter-attacks if the Carthaginians should try to erect their engines in it.

Thirdly, at this very moment the catapult was invented at Syracuse. Diodorus ascribes the invention to 399 (xiv, 42), and this date exactly fits the structural evidence. Euryalus, advanced in construction but still incomplete, was remodelled to mount catapults, and became the first artillery fortress in history. Five of these engines were placed on high towers behind the main defences, and swept with their fire the level ground in front. Dionysius's catapults were at first, it would seem, guns of position only. By 397 he was using them as siege-guns, mounted in emplacements and on ships ; but even then, at the siege of Motya, their function was only to clear the walls of defenders, not to batter the walls themselves (Diodorus, xiv, 51). It was not until a later date that they were made of such calibre as to be used in a battering-train—perhaps not until the time of Alexander the Great, who used them in this way at the siege of Tyre.

We must date the alteration in the plan of Euryalus, then, to 399 at earliest. Two years later, Himilco begin his campaign of re-conquest, and in 396 he attacked Syracuse. The storm which Dionysius had foreseen now broke ; but his preparations proved adequate. Himilco, encamped before Syracuse, failed to make any impression on it, and was finally driven off. It is surely evident that this was the reason why the second plan for Euryalus was never finished. The hornworks were only begun and the outer ditch only three-quarters dug when the alarm came ; and the fort was patched up into a defensible state by walling up the intervals between the artillery-towers. Even so, it sufficed. In those terrible years when almost all the chief Greek towns in Sicily were blotted out in blood and fire, Syracuse survived, saved by the

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military genius of Dionysius to carry on the torch of Greek civilization in the west.

II. SELINUS

The essential features of Euryalus are a scarped plateau, accessible to siege-engines only along a narrow neck of land ; a deep, flat-bottomed ditch dug across this neck and commanded by a gallery or covered way in its bottom ; and a series of towers for artillery, sweeping the open ground beyond it. These features all reappear at Selinus, and, so far as I can find, at no other site in Sicily or outside it. It is therefore generally allowed that the fortifications at these two places must have a common origin.

The town of Selinus has an hour-glass or spectacle-shaped plan. The southern lobe is the acropolis ; it ends to southward in cliffs standing over the sea, and its edges are everywhere abrupt, though not so steep as those of Epipolae. The northern lobe is rather larger and rather less steep-sided ; and the two are united by a level neck of land 70 metres wide.

The fortifications of Selinus are complicated, and present many problems which need not be discussed here. A simple wall, 2·1 metres thick, seems to have run all round the northern town ; but the acropolis was terraced to east and west by a massive and beautifully-built retaining wall, still standing to a maximum height of 9 metres. Monsieur Fougères would date this '*oeuvre colossale*', as he rightly calls it, to about 580, nearly half a century after the foundation of the town. To the south, along the sea-cliffs, the great terrace-wall is replaced by a simple structure 2-2·1 metres thick, like the wall of the northern town. Along the east side of the acropolis an outer wall has been added at some later date, running parallel to the great terrace-wall and close to it ; Monsieur Fougères points out its resemblance to the '*fausse-braye*' of medieval fortification, itself derived from eastern concentric fortresses like the wall of Constantinople. The Selinus '*fausse-braye*' is, by its construction, certainly Greek, but I am not prepared to suggest a date for it. Finally, there are in many places remains of a much later wall following the line of the original acropolis fortifications ; it overlies a foot or two of soil, and one can hardly be wrong in calling it Byzantine, as Monsieur Fougères does, or roughly medieval. I have not marked this wall in the plan here reproduced.

Of all this I shall say no more ; my business is with the northern defences of the acropolis. Here, the fine masonry of the early sixth

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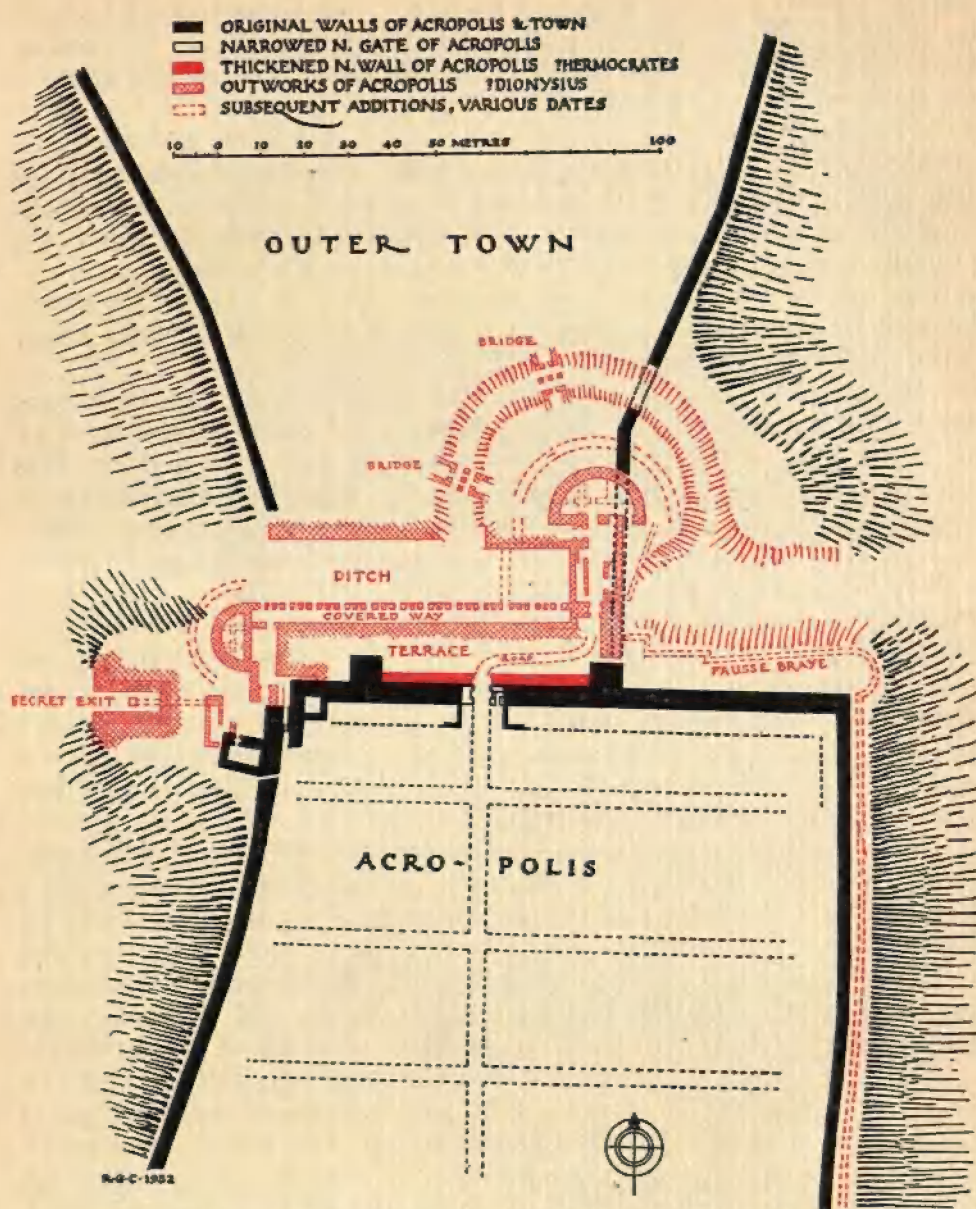


FIG. 3. SELINUS: plan of outworks at the junction of the acropolis and the outer town (some of the late additions are omitted)

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century wall—accepting Monsieur Fougères' date—runs straight across the neck, forming a curtain 4.25 metres thick with a gate 8.8 metres wide in the middle. Thin return-walls on either side of this gate, in the same masonry, seem to have served as revetments for an earth bank behind the wall. To east and west of the gate, and about 20 metres away from it, are square towers which I took to be contemporary with this original wall, but I find that Monsieur Fougères regards them as additions, and the mistake is more likely to be mine than his. At some time subsequent to its original building, the gate has been narrowed to 2.95 metres by adding cheeks at either side; at a later date again, the wall has been strengthened by adding a facing, 2.1 metres thick, running from tower to tower.

In front of this wall, and separated from it by a berm or terrace about 16 metres wide, is a ditch. Blown sand has obscured some of its features; but it was about 12–15 metres wide and 4 deep, flat-bottomed, and with vertical masonry sides. Along its south side runs a gallery level with its bottom and opening on it by twelve doors; between each door and the next are two loopholes permitting defenders in the gallery to sweep the ditch-bottom with archery fire. The gallery is reached by stairs and a passage at its west end, where a postern leads into the town; it terminates in a semicircular tower commanding the entrance to the ditch and the slope that falls away to the west. From behind this tower a second postern gives access to the country outside; and, as if this were not enough, an underground passage leads to a secret exit some 25 metres away from the wall, concealed in a sunken area with masonry sides. (Plate III).

The ditch is crossed by a solid causeway terminating in a hornwork. This consists of a second semicircular tower, defended by a ditch. The road into the fortified acropolis crosses this ditch by either of two drawbridges, bends round the western corner of the tower and passes through a gate, after which it traverses the causeway between mural chambers, turns on to the berm, and so reaches the main gateway. A postern leads out at the eastern side of the hornwork.

All this complex of outworks—ditch, causeway, gallery, and two semicircular towers—is of one build and forms one coherent plan; and the plan is in principle that of Euryalus. The towers can only be understood as batteries or emplacements for artillery. One covers the main approach from the north; the other, the western slope by which assailants might bring up their engines into the ditch. Instead of five small towers each mounting one catapult, we have two large ones

PLATE I



EURYMACHIA FROM THE WEST

The five towers (the fifth just begins at the right of the plate) have been built standing free: the masonry between them is later (note the difference of the stone). The masonry of the inner hornwork is visible in front and on the left; on the right, the partly-demolished first-period wall is seen running almost towards the spectator. In front of all, the inner ditch.

PLATE II



EURYALUS : OUTER DITCH

Looking northwest from the point of the outer hornwork. The figure, almost on the lip of the ditch, gives the scale.

PLATE III



SELINUS: THE COVERED WAY

Looking west. On the left, the terrace or berm; on the right, the ditch, its bottom level with the floor of the covered way and reached through doors. At the far end is the back of the western semicircular towers.

PLATE IV



SELINUS: NORTH GATE OF ACROPOLIS

Looking north. Note the added cheeks narrowing the gateway: in the eastern one, a door leads into a mural chamber. The rough masonry on the top of the wall is the 'Byzantine' wall. Houses on either side of the street in the foreground; its paving hidden by blown sand.

TWO GREEK FORTRESSES IN SICILY

each mounting a considerable number, and so placed as to cross their fire. The scientific use of artillery was far better understood at Selinus than it was at Euryalus, where the five catapults in a row were comparatively powerless to 'search' the neighbouring ground; for instance, only one could be brought to bear on a column attacking the town gate north of the fort. Clearly Selinus is a more developed and later plan than Euryalus. But, if the artillery defence fails, and the enemy effects a lodgement in the ditch, he can be attacked and his engines destroyed by means of the covered way; while an assault on the weak northwest corner of the acropolis can be taken in rear by a party using the secret exit. Further, these works are not only of one build, but they contain great quantities of reused stone from ruined temples, which suggests that they were built after the destruction of Selinus by the Carthaginians in 409.

There are many structures, added later, which modified this plan of fortification. The 'fausse-braye' which I have already mentioned begins at the causeway and includes a third semicircular tower at the northeast corner of the acropolis, matching that at the northwest; but this all seems to me a subsequent addition. So, even more certainly, are various other walls some of which are shown in broken lines on the plan. Tactically, all these are unimportant in their bearing on the general character of the works.

History tells us that Selinus was stormed and destroyed by the Carthaginians in 409. They are said to have brought up at various points six towers and six rams, with which they breached and entered the walls; and it is a fair assumption that, however they entered the outer town, they must have reached the acropolis from the north.¹ Next year, the deserted city was reoccupied by Hermocrates, the 'hero' (newspapers would call him) of the Syracusan defence against Athens. Hermocrates, whose portrait stands out from the pages of Thucydides with the brilliance of a Velasquez, was a fine soldier and a fine statesman. He saved Syracuse from conquest by Athens, and Syracuse repaid him with exile. He had correctly judged the Athenian threat to Sicily, and now he correctly judged the Carthaginian. Selinus had always been the outpost of Hellenism against the Carthaginian west, and Hermocrates saw that it must be revived. He went there

¹ M. Fougères places their main assault on the north front of the acropolis; but is he not forgetting that the outer town lay here, and must have been the primary object of attack?

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with 6000 men, and, we are told, 'refortified a part of the city' (Diodorus, XIII, 63), but fell next year in an attempt to re-enter Syracuse. Among his followers was one Dionysius; wounded and left for dead in Syracuse market-place, he survived to become tyrant and saviour of Syracuse and to marry Hermocrates' daughter.

If we ask what part of the city Hermocrates fortified, the answer is plain: the acropolis. Can we credit him with the elaborate outworks on its north side? He was a man of the highest intelligence, experienced in siege-warfare, and bent on securing Selinus against the Carthaginians, who had already once taken it in the teeth of a garrison far stronger than his own. Moreover, he was the master of Dionysius. Might he not have been the real inventor of the system which Dionysius used at Euryalus?

It is tempting to answer this question in the affirmative. *A priori* objections, as that Hermocrates had too few men and too short a time for so elaborate a work, are inconclusive; and the negative answer entails two awkward questions: first, if Hermocrates did not build these works, what did he build? and secondly, if Hermocrates did not build them, who did?

But the affirmative answer—already rejected by Monsieur Fougères, though on grounds not all of which are cogent—is, I think, ruled out by our analysis of Euryalus. If the defensive system common to the two places was applied to Euryalus after 402 only tentatively, with afterthoughts and alterations, it cannot have been borrowed from Selinus and embodied there in its fully-developed form by 407. A careful comparison of the two fortresses, carried out on the ground itself, will, I think, convince anyone that Selinus is an improved version of Euryalus in its final state. The system was invented at Euryalus, and then adapted to Selinus.

What, then, did Hermocrates do at Selinus? He no doubt repaired the walls of the acropolis; he may also have thickened its north wall, though of this I am not sure. Perhaps he only narrowed its gate. It is also very likely that he dug a ditch across the neck of land, obliterated afterwards by the outworks now visible. (Plate IV).

Our authorities tell us nothing of any fortifications by Dionysius at Selinus; but they do tell us that he reoccupied it in his Motya campaign, and had to give it up by treaty in 383. Monsieur Fougères is surely justified in contending that this occupation gives the date to which the outworks of Selinus must be assigned. It is not clear that they were ever completed. Monsieur Fougères inclines to think that

TWO GREEK FORTRESSES IN SICILY

the main gallery was never finished and never used : or at least that its roof was left incomplete, though admittedly it might be used in that condition.

The system of fortification invented, as we may conclude, by Dionysius I and used in these two fortresses never seems to have been used elsewhere. This cannot have been because it was found ineffective ; for undoubtedly it saved Syracuse, and Selinus only reverted to the Carthaginians by treaty. Nearly two centuries later Marcellus still regarded Euryalus as impregnable (Livy, xxv, 25). But the new fortresses of the generation succeeding Dionysius—Messene or Megalopolis, for example—show no attempt to follow his lead, and the general principles of Hellenistic and Roman fortification developed along other lines.

The reason for this, one may suggest, was twofold. First, the Dionysian method was far too elaborate to be used except on sites of a very special kind : sites naturally defended against the approach of siege-engines except on a single narrow front. Syracuse and Selinus are of this kind, but most towns are not ; and to throw a complete ring of batteries and covered ways round a large Hellenistic or Roman town accessible from many sides was impracticable. And secondly, the method was perhaps disproportionate to the danger against which it was a defence. We saw that Euryalus, even in its unfinished condition, withstood the Carthaginian assault ; and the fourth century experience of artillery seems to have convinced military architects that a plain wall provided with towers and mounting a sufficiency of catapults could be trusted to repel any ordinary assault. It was not until the invention of modern ordnance, with its greatly increased power of battering fortifications, that the principles first grasped by Dionysius were rediscovered and so developed as to lead to the creation of modern military engineering.

The Oxford Excavations at Hira, 1931

by D. TALBOT RICE

THE present age, as far as archaeology is concerned, may well be termed that of excavations, and Mesopotamia is more than any other country the centre of such activity. It is work on the most ancient sites that has come most before the public eye ; but later ruins too have received some attention, both before and since the war. Samarra stands and must long stand as the classical example of an excavation of the later period, but the recent work at Ctesiphon is no less competent and no less interesting. Greek Seleucia is now undergoing investigation by the Baghdad school and the Michigan University expedition and the ancient city of Kish has produced a Sasanian palace. Our work at Hira, some 50 miles to the south of ancient Babylon, now adds another name to this list, and though we made but preliminary soundings they served to show that the town of Hira was definitely an important and interesting place, the further examination of which will be without doubt well repaid. The work which we did was undertaken on behalf of Oxford University and was carried out under the joint direction of Mr Gerald Reitlinger and of the author of this paper. Most sincere thanks are due to Mr Reitlinger, who bore the entire cost of the expedition, and to Professor Langdon of Oxford, who greatly simplified our work by allowing us to become a branch of the Oxford-Field Museum expedition to Kish.

The city of Hira was founded sometime in the 2nd or 3rd century A.D., and for the next four or five centuries it thrived, not only as the capital of the Lakhmid kings, but also as a trading city and river port. We read that ships from India and China were even wont to ascend the Euphrates as far as Hira, where they discharged their cargoes for conveyance by land westwards and northwards. At a later date Hira became the centre, the very soul in fact, of pre-Islamic Arab art and literature. It was at Hira that the poets of the Age of Ignorance congregated and it was there that were nurtured many of the artistic manifestations of the Islamic period. It is thus to Hira that we must look for the origin of the idea of those desert palaces such as Mshatta

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and Ukhaidir, even if the actual edifices themselves are not so certainly to be attributed to Lakhmid influence.

In its earliest days, however, the town was not quite so impressive a place as it had become by the 6th century. When the city first began to assume any importance it must have looked rather like one of those Arab villages which border the cultivated land of Iraq today, the dwellings of the populace being composed of mud or reed huts and only the buildings intended for public assembly being of a more durable or more spectacular nature. The very name implies this, for the word 'hira' in Arabic denotes something in the nature of a settled camp, and at a later date it became associated with the palaces or hunting-boxes which were built in the desert by the early Mohammedan caliphs, nomads at heart, who used them as refuges to which to escape from the settled life of the towns or the cares and worries of government and officialdom. These hunting-boxes were, as already noted, a tradition from pre-Islamic times, and what may be termed the father of all of them, Khawarnaq, was situated only a few miles from Hira. It was built by one of the Lakhmid kings of that city as a resort for Bahram Gur, then the son of the Sasanian Emperor. The prince was brought up and educated at Khawarnaq and Hira, and it was his guardian, the Lakhmid king of Hira, who at a later date secured his succession to the imperial throne.

Hira did not remain a village for long, however, and early in the 5th century was sufficiently established a city to boast not only its own kings but also a line of Christian bishops. By the end of the century there were even two distinct Christian sects at Hira, the Monophysite and the Nestorian, though the former seems to have remained the more powerful, in spite of the fact that the ardent monophysite preacher, Simeon of Beit Arsam, succeeded in obtaining a large number of converts. In addition to Christians there must have been at least three other religious communities at Hira. The town was within the confines of the Persian Empire, so that the state religion, Mazdaism, must have boasted a few followers, and where Mazdaists and Christians were found together, there must certainly have been adherents of the Manichaean creed. In addition to these religions there must be counted the old pagan cults of Arabia, and the majority of the Lakhmid kings seem to have remained faithful to such deities. One of them even sacrificed four hundred nuns to the Old Arabian goddess Al Uzza, an event which can hardly have pleased his Christian subjects.

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The Lakhmid kings, although they were in theory vassals of the Sasanian Persians, were in fact more or less independent. They seem to have been supported by the Sasanians as a sort of buffer state between them and the Romans and Byzantines, just as these powers supported another Arab tribe, the Ghassanids, on the western side of the desert, to protect them from the direct attacks of Persia. But the Lakhmid kings not only protected the western frontier ; they also exercised a considerable influence in Persia itself. It seems probable that they eventually became rather too powerful, for in 602 the last of their line was deposed by Parwiz and a Persian governor was appointed in his stead. But the system thus inaugurated was not to last for long, for in 633 Hira capitulated, without any show of resistance, to Khalid on his first appearance in Iraq. The fact is surprising when we remember how close Hira was to Ctesiphon, the capital of the Sasanian Empire ; one would have thought that the Persian governor would have been able to put up at least some show of resistance. But it is explained by an examination of Hira's ruins, for there seem to be no traces whatever of a city wall.

From that day forth the standard of Islam was firmly planted in the region and though Hira continued to exist for another two centuries at least, during which time she probably remained a Christian city, her importance as a river port and as a trading centre gradually declined owing to the foundation of the new Moslem town of Kufa some four miles away. Hira's larger buildings were despoiled, so the Arab writers tell us, and the stones and bricks were employed at Kufa to construct the mosques and other new buildings of Islam. By the end of the 9th century there can have been little more than the mounds of mud and the heaps of broken bricks which survive today to mark the site of the ancient city.

Though none of the mounds of Hira are very high, the ruin-field is of considerable extent and it seems that throughout the ages the city, instead of rising up layer upon layer as did so many more ancient towns of Babylonia, gradually moved northeastwards, so that the present Kufa is some six or seven miles from the furthest extremity of Hira, where the oldest ruins lie. This distance is made up firstly by the ruins of Moslem Kufa, which extend to the southeast of the present houses for at least two miles, then by an open space and then by the ruins of Hira, which are again at least two miles across. The mounds of Kufa are bordered by the banks of a dry canal, beyond which is the open area, which was apparently never built over. It was at the

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edge of this area that we began our work, in a mound which we called number 1, and soundings were made in lines to the east and to the south of this point, extending in the one direction for about half and in the other for about one mile. These soundings were numbered in the former direction from II to VII and in the latter from VIII to XI. They showed us that the larger buildings had been dotted about here and there and that practically all the remainder of the whole area had been occupied by small houses of a single storey. A number of these were investigated and some of them proved to be quite elaborate, their doorways being adorned with jambs of finely carved stucco.

In addition to these houses, three larger structures were examined fairly thoroughly. These were mound 1, mound v, and mound xi. The two latter turned out to hide the remains of churches, both of which probably date from the 6th century, though they had undergone repairs more than once at subsequent dates. A few lines of Kufic scratched on the walls of one of them prove that it was still in use after the Moslem conquest. These churches are long, basilica-like edifices. One of them, number xi, had three aisles, separated by circular columns of baked bricks. (Fig. 1). The walls are of mud, with pilasters of burnt bricks against them; the floors are of baked brick, set diagonally (plate 1); the aisles were roofed by vaults. In building xi each aisle apparently had a separate barrel-vault above it, and the other church, number v, was roofed by a single span. At the east end there were in both churches three chapels of similar plan, their eastern walls straight and not apsidal. The rectangular plan of these chapels (figs. 1 and 2) is quite distinct from that of the usual Syrian or Coptic type of building, where the semicircular apse is characteristic, and it seems probable that the influence of the rectangular temple-courts of Babylonia and Assyria is to be traced here.

The type of church which we see at Hira seems to be a distinct one which was characteristic of southern Mesopotamia, for the German expedition at Ctesiphon found a church which is in plan practically identical. It too was roofed with a brick vault, and its doors, like those of the churches at Hira, were small and narrow and were placed at the sides. In Egypt, Butler notes that Coptic churches were invariably designed to have triple doors at the west end, though in actual fact the doors were often put at the sides owing to practical necessity. In southern Mesopotamia it seems that the side doors are an original and an intentional feature and not one forced upon the builders by exterior circumstances.

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A further characteristic feature of the churches of Hira is a raised platform and screen at the centre of the nave, of which traces only remain in church number v, but which was perfectly preserved in number xi. A bench occupied either side of the platform, which must have served as a kind of throne for the elders. The screen, which stood to the height of a man's elbows, was immediately behind it

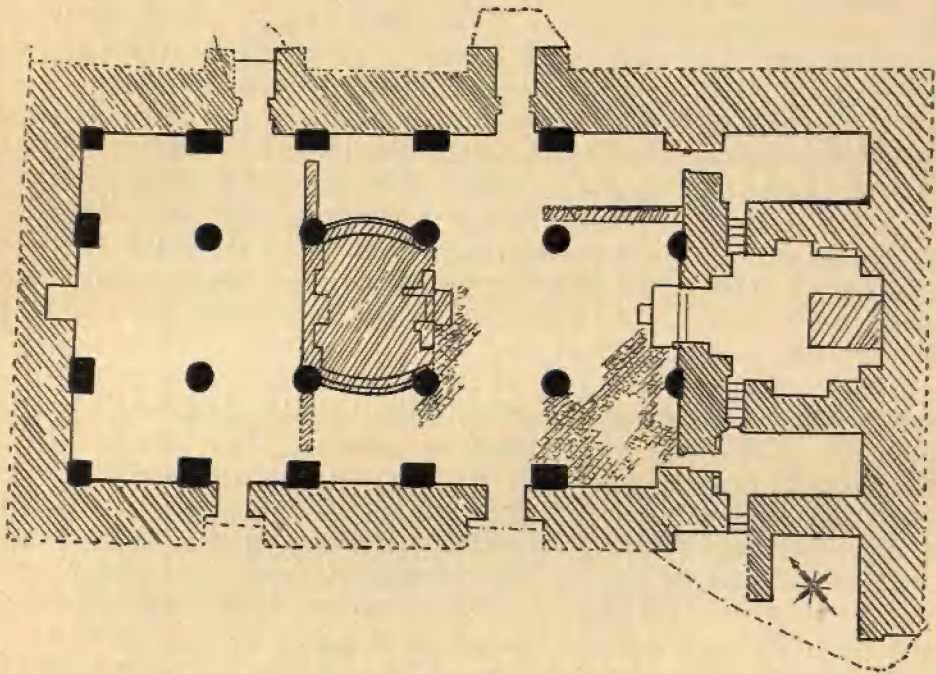


FIG. 1. CHURCH (No. XI) AT HIRA
 ■ Baked Brick. ≡ Mud Brick. //// Subsidiary Construction.
 - - - - - Limit of Excavation. - - - - - Restored. — = 3 metres

(fig. 1). It may have served to separate the men's portion of the church from that reserved for the women, a division which also appears in the Coptic churches of Egypt.

The walls of our churches were covered with white plaster—as many as five layers laid on at different periods of repair were preserved in building xi—and in the eastern chapels elaborate wall-paintings were executed. A cross painted in red survived in the southern chapel of church xi (plate II). In the central chapels of both churches numerous fragments of painted plaster were found; a number from church xi

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could be pieced together to form a cross, which is now on exhibition in the Ashmolean Museum at Oxford. One fragment bore what may have been the figure of an 'orante', though this is the only indication that there was anything in the nature of representational art at Hira. In both our churches there were paintings of more than one period. The floors of the central, but not of the side chapels, had been raised

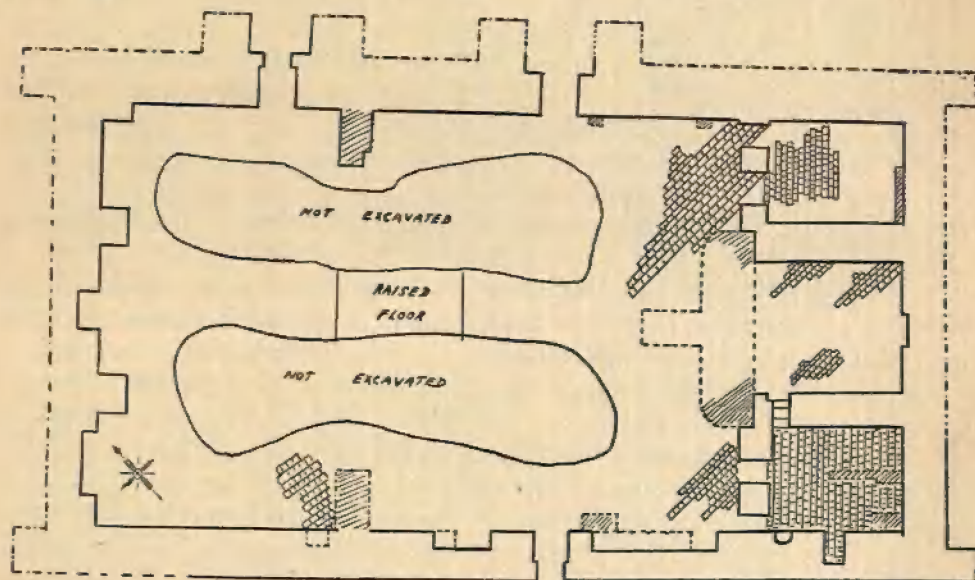


FIG. 2. CHURCH (No. V) AT HIRA

— Original building. - - - Repairs or modifications. - - - - Restored or not excavated.
 //// Baked brick. ————— = 5 metres

at times of restoration and some fragments of paintings of an early date were found in the filling below the upper floor in church XI. In church V some of the early work remained on the walls, obscured by an upper layer. We were able to remove this and though the colours of the earlier paintings were very faint, enough remained to show that the work was of a much more Sasanian character than that above it, the colouring and style of which was more akin to that of the Moslem paintings at Samarra. The importance of these early paintings is considerable, for it is to the Christian paintings of Mesopotamia that we must look for the early stages of Islamic book-illustration, and it seems in every way probable that there may lie hidden below the soil

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of Hira numerous examples of a more complete character than those which we found last winter. Mosaics too may await the explorer, for numerous tesserae, both of marble and of glass mosaic, were collected on the surface by our workmen.

In addition to the paintings, fragments of some small plaques of very fine plaster-work, bearing designs of which crosses are an essential part, were found in the filling of the churches. They are of two types, the design being either incised and coloured in red (fig. 3, a and b) or else left in relief, the surrounding ground being cut away (figs. 3 and 4). These crosses are of a distinctive type, the leaf pattern at the base being especially characteristic (fig. 4). A close relationship with crosses sculptured in stone which appear in Armenia about the 10th century seems certain; it is illustrated by a comparison of examples in relief (fig. 4, a and c), with such crosses as we see on the walls of the famous 10th century church on the Island of Achthamar in Lake Van (fig. 4, b).

In discussing the elaborate carved ornament which we see developed to such an amazing extent in Georgia and Armenia between the 10th and the 14th centuries, Baltrusaitis, in his book *L'Art médiaeval en Géorgie et en Arménie*, concludes that there is in Armenian work a great deal of Arab influence. The essentially mathematical character of this ornament seems to have appealed especially to their particular genius, he notes, and though the origin of many of the actual motives is to be sought in the earlier arts of Persia or Mesopotamia or even of Hittite Asia Minor, the spread of these elements is due to the Arabs. And here at Hira we see these crosses developed by Arab Christians on the age-old soil of Mesopotamia, several centuries before the earliest examples of which we know in Armenia. It is to such examples as this that we must look for the parentage of Armenian art and our Hira plaques are hence of the first importance. They are in addition very attractive little things, constituting in themselves minor works of art. They must have served as small 'icons' rather than as 'tiles' for attachment to walls, and the fact that all our examples have their edges rounded off shows that they were of a portable nature. They bear a purely formal ornament, nothing in the nature of figural art appearing, so it may be that we have here an example of that early non-representational art which Strzygowski regards as characteristic of the early Christian period in the Near East.

Our building number 1 was definitely of a secular character, and it appears to be, in part at least, of Moslem date, though the original



FIG. 3
DESIGNS ON PLASTER WORK, HIRA

FIG. 4
CROSSES AT HIRA AND ACHTHAMAR

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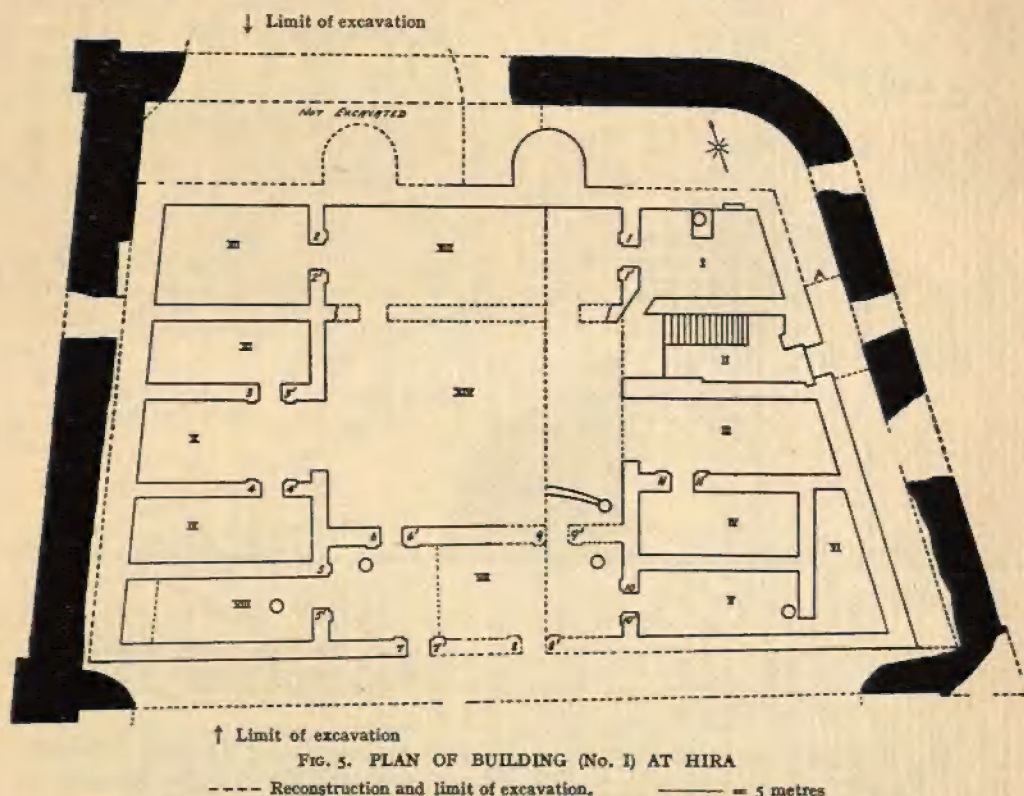
plan is earlier. We were able to clear it more or less completely, so that a full plan could be made (fig. 5). It proved to be a fairly large construction with fourteen rooms and it must have served as a fort, small palace or large house. It had undergone repairs and had been reoccupied at various periods. The building stood to a height of two storeys, the walls of both following exactly the same plan. But those on the lower level had no doorways in them, so that the rooms must have served as cellars, accessible from above. At the period during which the floors of the upper rooms were last in use, however, the cellars had been filled in with sand and earth and the floors of the rooms rested upon this filling. Below these floors large pottery vessels had been inserted in places, their tops being bricked over. They must have been used as drains, the water soaking out through the porous clay into the surrounding soil (plate III). These large vessels are definitely of Moslem date and some small fragments of pottery which were collected from the filling of the cellars are akin to examples from Samarra and elsewhere, dated by Sarre to the 8th and 9th centuries. Coins from the filling of the upper rooms belong to Caliphs of the 8th century.

Our upper storey then, or anyhow the floor of it, is to be dated to this period. The entrance at this date was on the south side and it seems that the level of the surrounding soil was here already about two metres above plain level. A few steps only would thus be necessary to reach either of the doors which are marked on the plan. Today the soil has been so much washed down on this side of the mound that no traces of the approaches to these doors remain.

If the last period of occupation was in the 8th century, the first was certainly well in Sasanian days, though there is unfortunately no evidence to tell us the exact date. To this first period we must assign the large outer wall of baked bricks, measuring 26 by 26 by 5 cm., which is marked in black on the plan (fig. 5). It is the wall of some important building, to which the mud brick walls of the later structure within have had to adapt themselves, though on the south side these walls are actually built upon the baked brick wall. But our building within does not belong to the late date at which the large pots were inserted below the floors, for its original entrance was on the lower storey, in the corner of room II, and from here a stair, remains of which still exist, led to the upper level. Opposite the foot of the stair was a door which gave onto the space between the outer wall of the building and the baked brick wall, through which there was a second door, reached after a slight deviation (plan, fig. 5). This door was bricked up, and

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indeed the whole space between the surrounding wall and the outer wall of the actual building was filled in with mud bricks, presumably when the cellars became filled with sand. (Plate IV). The arrangement of the various walls can be most clearly seen in the section, taken at the corner close to room II (fig. 6). The bricks of all three walls are of different sizes and a fourth type of brick is encountered within, for



the doorways of the upper rooms were edged with small yellow baked bricks, each 18 by 18 by 4 cm. Such bricks have hitherto been considered of rather late date, but there can now be no doubt that they were used as early as the 8th century; they may even have been employed during Sasanian times.

Our building hence belongs to three main periods. To the earliest is to be assigned the surrounding wall of baked bricks, each of them 26 cm. square. To the second are to be assigned the walls and

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plan of the building within, the bricks of which measure 35 by 35 by 7 cm. To the third belongs the filling of the cellars, the building up of the space between the walls (bricks 27 by 27 by 11) and certain repairs in the walls of the upper rooms, if not the actual walls themselves. And it seems that the elaborate door-jambs of carved stucco, some of which remained *in situ* (plate v), must also be assigned to the latest period.

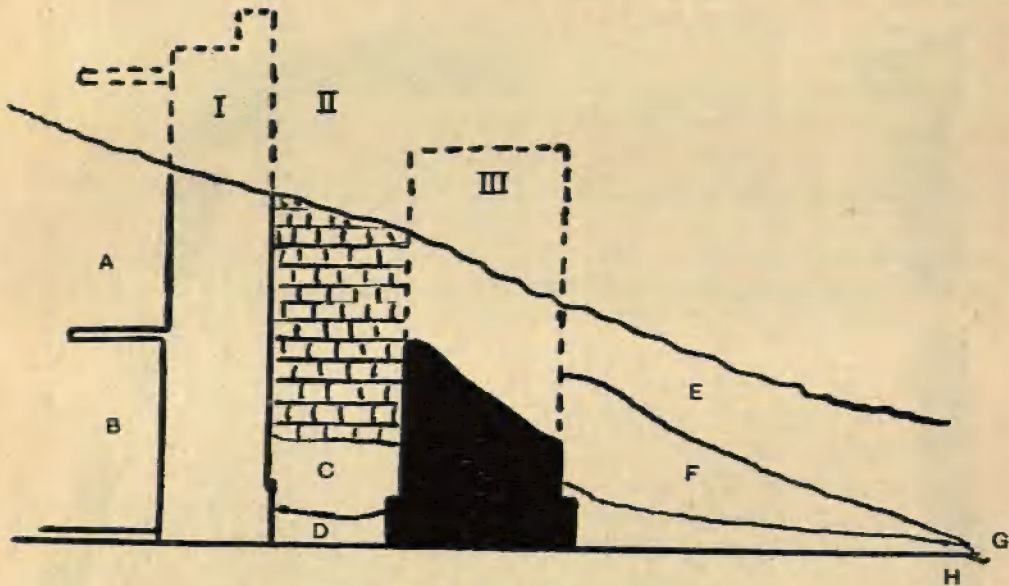


FIG. 6. SECTION OF BUILDING (see plan, fig. 5)

Legend. I, inner wall, mud bricks. II, filling wall, mud bricks. III, outer wall, baked brick
A, upper rooms. B, cellars. C, filling of pounded earth, etc. D, filling of concrete. E, deposit, washed down from above. F, deposit of bricks and cement, fallen from wall III. G, deposit of cement, etc. Fragments of carved and painted plaster from here. H, natural sand

On the northern side of the building two semicircular turrets project from the wall (plan, fig. 5). Such turrets are characteristically Sasanian and their presence here suggests that our second period, to which they belong, is to be placed in Sasanian times, or at any rate only shortly after the Moslem conquest.

The sculptured door-jambs of building I were all different one from the other and in mounds III and IV further examples of similar type and date were also unearthed (plate VII). The majority are deeply carved; they show very elaborate workmanship and a considerable diversity of motives. The fine example from room 13 of building I shown in plate v was balanced at the opposite end of the room by the

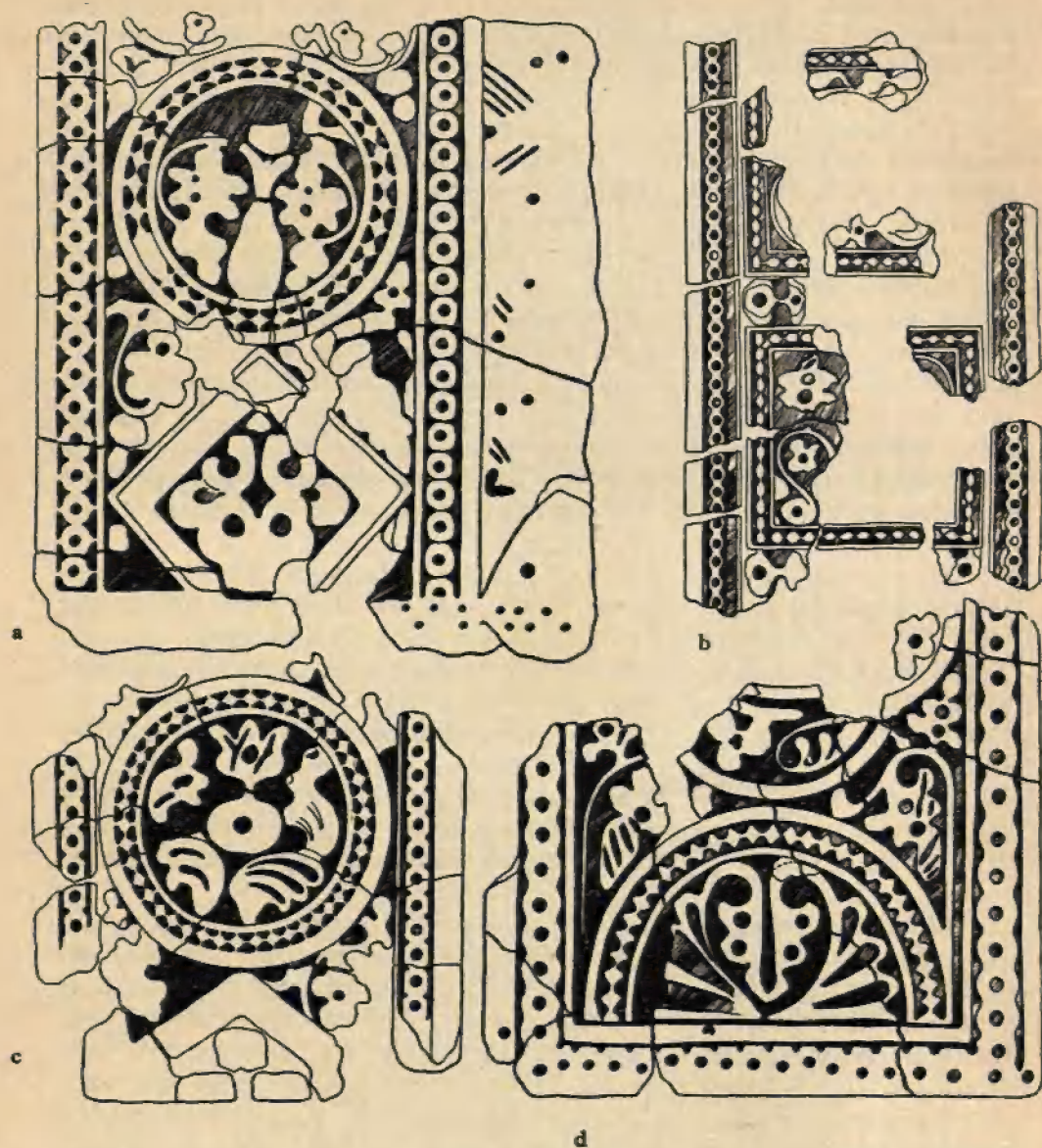


FIG. 7. SCULPTURED DOOR-JAMBS AND DECORATION AT HIRA

specimen shown on plate VI, the larger section of which was retained by the Baghdad museum. The looped motive is peculiarly suggestive of 'Celtic' ornament. Other motives of decoration on panels found in building I are shown in fig. 7, c and d, while plate VIII and fig. 7, a and b illustrate portions of door-jambs found *in situ* in the small houses of mounds III and IV, which bear palmettes or similar motives, often of a rather debased character. Figures 8a and b show fragments of similar door-jambs collected from the floors where they had fallen when the walls collapsed. Portions of two cornices reconstructed from fragments collected from the floors are illustrated in figs. 8c. These either served to top the doors or else they ran round the rooms at a given level. In building I, however, it seems that some of the doors at least were topped by arches, and fragments of similar arches were found on other mounds (fig. 8d, e and f). A number of fragments of a rather more delicate character are shown in fig. 8g-k, some of the motives being definitely based on the cross. The small pilaster (g) may perhaps have served as the frame to some sort of 'icon', such as those described above.

It is interesting to compare these stuccos with other examples known from elsewhere. Most closely akin is a door-jamb which was found by the German expedition at Ctesiphon a few years ago and which is dated to the 9th century. The ornament is practically identical with that of a door-jamb from one of the small houses at Hira (fig. 7, a), though our example is dated to the 8th century by stratigraphical evidence. Less closely, but still closely enough related, is the carving of part of the wooden mimbar at Kairouan, which is, according to Strzygowski, of Mesopotamian workmanship. It is of course far finer than anything from Hira, but the close similarity of many of the motives cannot be denied.

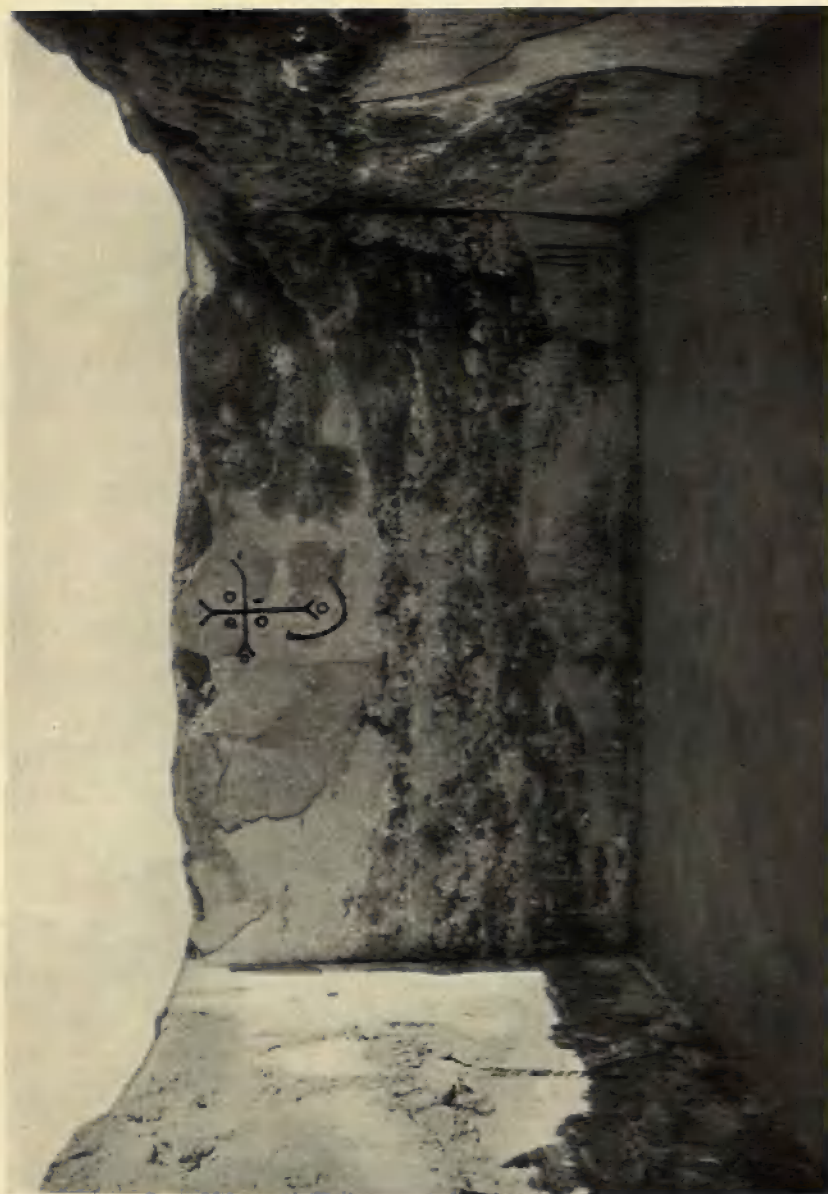
On Persian soil the exquisite work at Nayin which belongs to the 9th century shows motives which are closely related, though they are fairly obviously of later date, and a further development of the style is shown in the fine mihrad of marble at Cordova carved about 970. In Egypt the 9th century stuccos of Deir es Suryani illustrate a parallel development. Coptic grave-stones of the 6th century from the same country would on first sight again appear to be related, their plaited borders being well-nigh identical with those at Hira. But in this case the motives of the main decoration are dissimilar, and as the plaited band is an old Assyrian motive there seems no reason to postulate any very close relationship with Coptic Egypt. It is on the evidence of

PLATE I



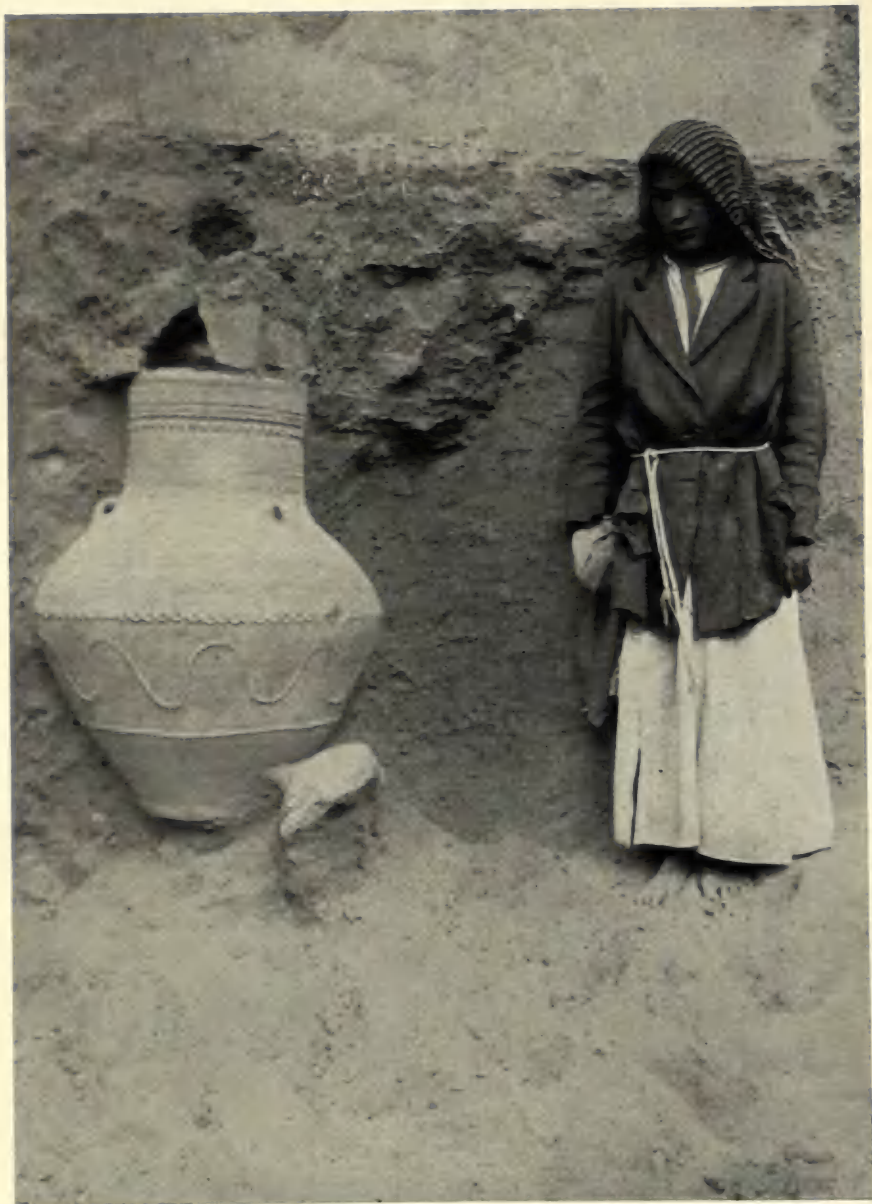
HIRA: CHURCH XI, LOOKING EAST

PLATE II



HIRA : CHURCH XI, CHAPEL OF THE SOUTH AISLE

PLATE III



HIRA : LARGE POT BELOW FLOOR OF BUILDING I

PLATE IV



HIRA : MAIN DOORWAY, BUILDING I

PLATE V



HIRA : STUCCO DOOR JAMBS, BUILDING 1

PLATE VI



HIRA : STUCCO DOOR JAMES, BUILDING I

PLATE VII



HIRA : SCULPTURED DOOR JAMES, MOUND IV

PLATE VIII



HIRA: STUCCO DOOR JAMB, MOUND IV



FIG. 8. FRAGMENTS OF DOOR-JAMBS, CORNICES, Etc., HIRA

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such grave-stones that Butler suggested a Coptic parentage for some of the Samarra pottery.¹ But the motives he cites would mostly seem to be of a Mesopotamian heritage and we see them in use at Hira a short time before they appear at Samarra and in Sasanian art at an earlier date still. It is to this earlier Sasanian art that we must turn if we seek to trace the parentage not only of these motives, but also of the Hira stuccos, and recent excavations have furnished us with quite a considerable amount of material. Of this perhaps the most striking is the carved ornament and figure sculpture of early Sasanian date found at Kish in 1931.² Pomegranates, palmettes, foliage and other motives which appear here are the obvious parents of many of the Hira motives, though at this later date they are depicted in a somewhat less pure and less precise manner. The same motives, and in addition bands composed of studs or loops, appear on stuccos from Ctesiphon which are to be dated to about the 7th century. Examples published³ show that though some of the fragments are more Hellenistic in style and are very obviously earlier than the Hira work, others are more closely akin and would seem to be direct and not very distant ancestors.

Closely related again are stuccos found this winter by Dr Schmidt in a palace at Damgan in northern Persia.⁴ We see here palmettes, acanthus and other motives which are obviously related to our Hira material. The Damgan stuccos are as yet undated, but they would appear to be fairly early Sasanian, belonging perhaps to the 4th century. They show Hellenistic influence to a marked degree.

The stuccos constitute the most bulky of the finds from Hira, but certain other minor objects are in reality no less interesting. First of these come the small plaques described above, the importance of which in the history of Near Eastern art we have already stressed. Next perhaps may be considered the glass, though few complete specimens were found. The necks of some small jugs, decorated with bands embossed in different colours, show extremely delicate work. Fragments of glass lamps, shaped like champagne glasses from church v, appear to be very closely related to types of Byzantine date from Jerash and other sites in Syria and Egypt.⁵

¹ *Islamic Pottery*, p. 45. He illustrates a Coptic grave-stone on pl. 28.

² Published in *The Illustrated London News*, 14 February and 7 March 1931.

³ *ANTIQUITY*, 1929, III, 44, pl. v.

⁴ *Illustrated London News*, 26 March 1931.

⁵ Crowfoot and Harden, 'Early Byzantine and later Glass Lamps', *Journ. of Egyptian Archaeology*, 1931, XVIII, pts. 3-4, p. 196.

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Pottery fragments were fairly abundant. Of the glazed specimens most come from near the surface ; they belong to the Moslem period and are akin to types known from Samarra, Susa and elsewhere. A fine blue-glazed pot more or less complete with an impressed decoration, from a room in one of the small houses, is probably to be dated to Sasanian times. Some small pieces of unglazed ware from the filling of the cellars of building I would also appear to be Sasanian ; others belong to known types of 'barbotine' ware and are definitely of Moslem date. A well in mound II provided us with numerous sherds of commoner pots, which bear a graffito ornament around the neck and shoulder. Some of the motives, which consist of combinations of circles, curves and hatching, are quite attractive and they often suggest a survival of the motives of early times.

The Hira excavations were thus fairly productive as regards material. But they also furnished us with a considerable amount of information about a period of history of which we know very little. They give us some clue as to the nature of the life of a town which was for at least three centuries one of the most important cities of Arabia and Mesopotamia ; they tell us of the early spread of Christianity in the East and they throw a certain light on some of the more vexed questions in the history of Eastern and indeed, in the whole of Christian art. Yet work was carried on for a month only, and we were unable to make more than soundings. We did not touch any of the earlier ruins of Hira ; this remains for a future season, and it is to be hoped that funds will be forthcoming and that it will be possible to continue the work so admirably begun by Mr Gerald Reitlinger.

The Paradox of Celtic Art

by R. E. M. WHEELER

IN the June number of *ANTIQUITY*, Mr T. D. Kendrick sketched what in another sphere of art might be called a Conversation Piece. His subject was the family of hanging-bowls, and he assembled it with skill and daring in a new setting. His method was frankly impressionistic, and was proportionately stimulating. With the aid of a partly theoretical chronology, he inferred 'that in origin these bowls are really Romano-British; that many of them had been made and were in use before the Romans left this country; that others were made after the Romans had gone, and belong to the almost unknown archaeology of the Arthurian period'. To these conclusions it might at once be objected that, amongst the bowls, no dated example is of Romano-British period, and that they hardly occur in Arthurian Britain. But rather than press these objections, let us explore an alternative interpretation. Let us first state the problems and the relevant facts; then attempt to reconcile the latter with the former. This reconciliation will be no easy task, but with faith and hope on our side, and charity on Mr Kendrick's, it may at least be essayed.

Two main phases of Celtic art are recognized in the British Isles. The first (Pagan Celtic) begins in the fifth century B.C., and, apart from very rare survivals, ends in the second century A.D. The second (which may be roughly though incorrectly called Christian Celtic) begins about the sixth century A.D., and lingers on until about the ninth century. These two phases share that bold use of eccentric curves and swelling forms which at once distinguishes Celtic alike from Classical and from Teutonic art; but they differ also from each other in a number of important features. These features are too extensive, and perhaps too subtle, for satisfactory generalization; but they may in part be summed up in the assertion that Pagan Celtic art derives a majority of its motives from the classical palmette, whereas Christian Celtic art owes more to the classical pelta-pattern or

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'Amazon's shield', which had an extensive vogue in late Roman and post-Roman times.¹

We are faced, then, with two individual phases of art which are at the same time linked by an essentially similar informing spirit and divided by a hiatus of three centuries of time. Such a hiatus is rare in the history of a single school of art, and some special explanation must be sought. It is not, in England at least, due to any lack of archaeological material within the missing centuries. Dated material of the third and fourth centuries, if not of the fifth, is abundant, and its negative evidence on this point is finite. A few scattered links may accumulate as time goes on: we have the semi-Celtic triskele on the Kyngadle patera, found with late third-century coins, and a bronze Celtic triskele from a fourth-century stratum at Verulamium (fig. 3). But strays such as these merely emphasize the barrenness of the land. There is in England a definite hiatus of three centuries in the history of Celtic art, nor is there at present any satisfactory evidence for this period either from Scotland or from Ireland.

But the problem does not end there. When Celtic art re-emerges after this long hibernation, it appears under conditions which are at first sight nothing less than astonishing. It appears first in the form of escutcheons on hanging-bowls which, when found in association, occur in pagan Saxon burials of the sixth and seventh centuries. Mr Kendrick has observed that about 15 of the bowls, or parts of the bowls, have been found under these circumstances, and that none has been recorded in any other association. Moreover, with the exception of a single outlier in Ireland and occasional strays in Scandinavia, where, it is agreed, they must be loot from Britain, these bowl-escutcheons are found mainly in the parts of England which were most intensively Saxonized. As Mr Kendrick's very instructive map (fig. 2) shows, a majority of them occur to the southeast of the Fosse Way.

Whatever we may like to think, therefore, these Celtic escutcheons belong to the Pagan Saxon period and to the Saxon areas of England. They are almost completely absent from those parts of Britain which, lying beyond the Saxon pale, must have remained dominantly Celtic. They are equally absent from the Teutonic regions on the Continent, and were certainly, therefore, not introduced by the invading Saxons. How are they to be explained? The paradox is a pretty one and

¹ For the pelta-pattern, see A. W. Clapham, *Archaeologia*, LXXVII, 227; and Wheeler, *Lydney Report* (Society of Antiquaries), p. 66.

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provokes discussion ; but we shall get no nearer to a solution of it if we attempt to monkey with these basic facts. They must stand at all costs.

An artistic impulse, like life itself, defies ultimate analysis. We can no more really explain the genesis of a thing like Celtic art than we can really explain the genesis of a bumble-bee. All that we can hope to establish is a particular environment, a concatenation of specially favourable circumstances, which may reasonably be thought to have contributed to the genesis or regeneration of Celtic art in the time and place where that event in fact occurred. Nor need we envisage a long period of gestation, particularly in the case of an art dependent, like Celtic art, upon entirely decorative and non-animate motives. Round about 400 B.C. the Celts of Central Europe snatched at the classical palmette and almost immediately twisted it into the fantastic volutes of Pagan Celtic art. They could not do otherwise, for they were as incapable of a literal imitation of the classical palmette as were the later Celtic moneyers of imitating the chariot of Philip of Macedon. Similarly in that second phase of Celtic art we need not look for a long period of laborious preparation : our enquiry may be restricted merely to a search for the propitious circumstance, which may in the end have arrived quite suddenly. We are not, incidentally, concerned with the general history of hanging-bowls, whether they held incense in prehistoric Flintshire or geraniums in modern Pimlico. We are concerned only with the Celtic artistry with which certain of them were adorned in England in the sixth and seventh centuries A.D.

I propose to begin with an analogy—to tackle the second Celtic phase from the vantage-point of the better-known first phase. In view of the essential artistic unity of the two phases, this procedure is likely to be more helpful than such analogies sometimes are.

Now, it is recognized that Pagan Celtic art reached its climax in England during the first centuries B.C.—A.D. And if one were pressed to restrict more narrowly the period of culmination in central and southern England, one might indicate the period A.D. 1–60 as that of the two most skilful generations of Celtic craftsmen. Further north, the flower bloomed somewhat later. To the first half of the second century A.D. belong those Celtic or semi-Celtic ‘ trumpet-headed ’ or ‘ harp-shaped ’ brooches which Mr R. G. Collingwood has discussed.² These facts are suggestive.

² *Archaeologia*, LXXX, 37.

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It is at once clear that the most luxuriant period of Celtic art coincides with the consolidation of political authority in the island, at first indirectly and later directly under the influence of Rome. In spite of romantic theories to the contrary, art does not flourish in a garret, particularly in an insecure garret. The golden periods of art have a solid bullion basis. Art demands both wealth and security—two factors which are, of course, closely interdependent. And in the period with which we are concerned, wealth and security began to come into southern England with the political and commercial expansion of the Tasciovanus-Cymbeline period, 30 B.C.—A.D. 40. The process was hastened and extended when the Romans came tumbling into the country in the year 43. For the first time, the whole of central and southern England was properly policed. Wealthy prospectors rushed into the promised land. For a moment the Celtic craftsman had such an opportunity as he had never had before. His technique was amplified by the addition of new enamels; patronage and leisure were his. Then the tide of Roman mass-production burst upon Britain and engulfed the individual Celtic crafts. Within less than a generation of the Claudian invasion, Celtic art south of the Humber was swamped.

Let us take one example. Amongst the most mature products of Pagan Celtic art from Britain are the engraved and enamelled mirrors, dated by the Birdlip and St. Keverne examples to about A.D. 50.³ These mirrors occur in Kent, Essex, Bedfordshire, Northamptonshire, Gloucestershire and Cornwall—*i.e.*, all, with the exception of a 'late and hardly typical' example from Kirkcudbright, to the southeast of a line from the Bristol Channel to the Wash. Their period is one in which co-ordination and discipline were being introduced into that very region of Celtic Britain successively by Romanizing Belgic potentates and by the Romans themselves. The parallel with the sixth and seventh century Celtic escutcheons is almost exact: they too, as already noted, occur mainly to the southeast of the Fosse Way, and their period is that in which this region was being consolidated by the Saxons. (Compare figs. 1 and 2).

The period of social transition during which the engraved Celtic mirrors flourished in southern Britain was a brief one. The latest dated example—an ancient export to Holland—belongs to the latter part of the first century and shows every symptom of advanced

³ Reginald A. Smith, *Archaeologia*, LXI, 329.



FIG. 1. Distribution-map of engraved Celtic mirrors of the period of Roman settlement (mid 1st century A.D.)
(Note: the Scottish example is a variant which does not affect the distribution of the main type)

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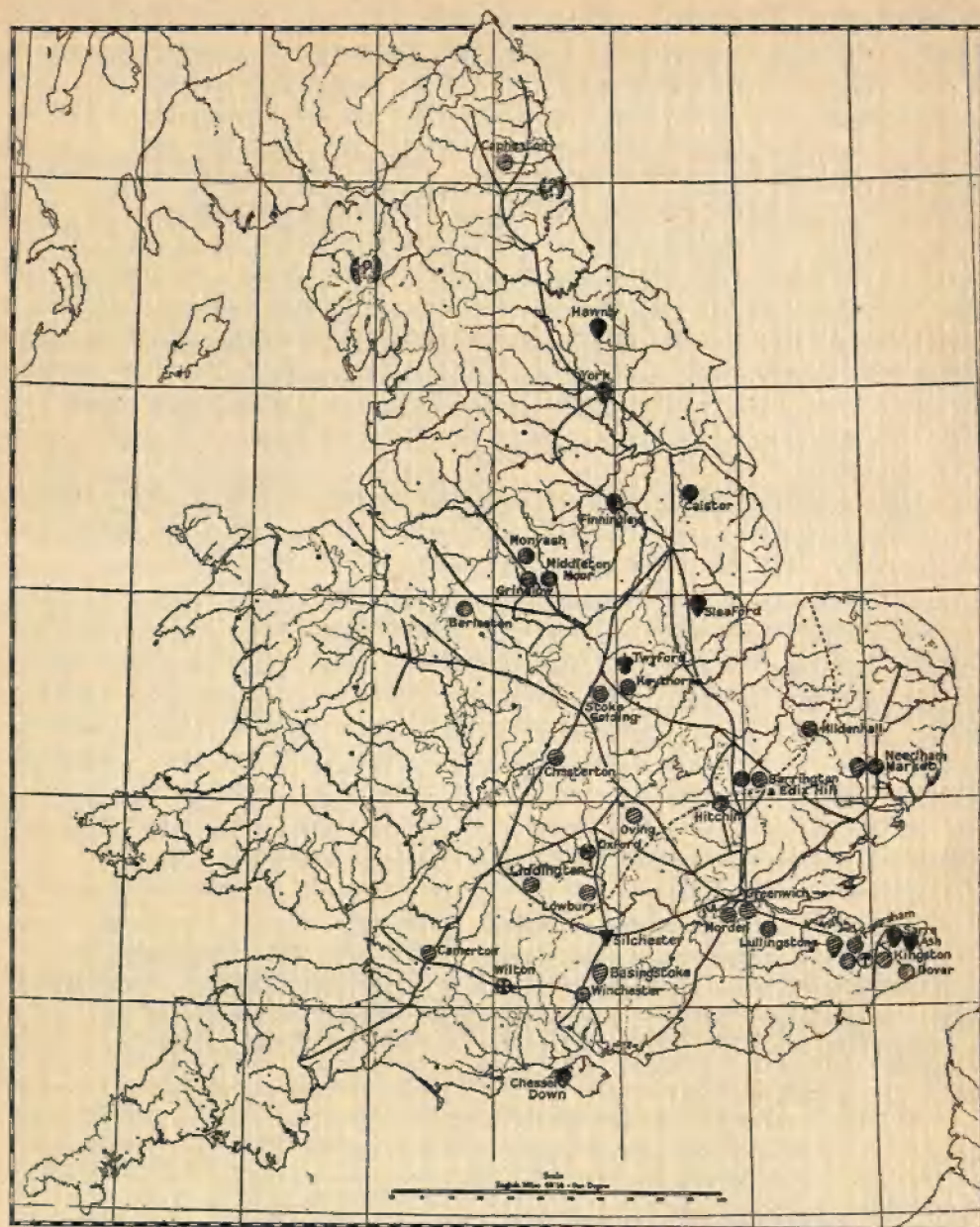


FIG. 2. Mr Kendrick's map shewing, by means of the *hatched* symbols, the distribution of enamelled bowls, mostly Celtic but of the period of Saxon settlement (c. 6th century A.D.). Compare with Fig. 1

decadence.⁴ But north of the Humber an analogous sequence of developments now ensued. Here prior to the Roman conquest there was no equivalent to the Belgic development of southeastern Britain. It was not therefore until the final pacification of the Border-lands at the beginning of the second century A.D. that the settled conditions favourable to artistic enterprise supervened. Then it is that we get that northern school of Romano-Celtic craftsmanship referred to above. The moment that a compulsory neighbourliness and permanent markets were established, the Northumbrian Celt set to work. And since the Romanization of his region was less intensive and universal than in the more fertile south, he was less speedily overwhelmed by Roman mass-production than was his Belgic confrère. For two generations he preserved not a little of his own Celtic individuality. Then he likewise began to loose his grip upon the finer craftsmanship, and others took his place.⁵

The lesson of all this is not difficult to read. The Celtic artistic temperament was a plant of tender growth. It flourished in moments of security, but its intense individuality (two objects with identical Celtic decoration are of the utmost rarity) prevented it from surviving in the presence of organized commercial competition. Now, history shows abundantly that political coherence and security were never strong points amongst the Celtic peoples. It is therefore entirely natural to find that Celtic art flourished most readily when political and economic security were thrust upon the Celts by some external power (the semi-Teutonic Belgic princes; the might of Rome); provided that this external factor was not associated with too easy an alternative (such as the mass-craftsmanship of Rome).

⁴ G. C. Dunning, *Archaeological Journal*, LXXXV, 69.

⁵ In the very interesting and important paper cited above, Mr Collingwood, discussing this northern Celtic art, over-emphasizes, I think, the significance of the supposed aesthetic factors and underrates the economic ones. He regards the 2nd-century Celtic trumpet-brooch of the north as 'a result of the stimulus given by classical art, even in a debased and mechanical form, to the British artist's mind'. This does not seem to me to fill the picture. As I state in the text, I prefer to look for the primary stimulus to a land flowing for the first time with police and money, but not yet flooded with intrusive mass-production. As soon as the northern countryside was organized and supplied by Roman mass-production, the native initiative lapsed. It is perhaps rather to this than (as Mr Collingwood suggests) to a hypothetical massacre of the Brigantes about A.D. 158 that the initial decline of northern Celtic art should be ascribed in the middle of the 2nd century; although a period of unrest, such as that which Mr Collingwood has in mind, would of course be equally destructive.

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We are now ready to turn from the pagan phase of Celtic art to the post-Roman or 'Christian' phase. Here it is necessary to recall two preliminary historical factors. In the disintegration of Romano-British life which followed the severance from Rome in 410, it is clear (in so far as anything in that period is clear) that the population reverted, at any rate in part, to conditions analogous in many respects to those which prevailed before the Roman conquest. Prehistoric hill-forts were reconditioned (Lydney, on the western shore of the Severn; Cissbury Ring in Sussex), and petty kingdoms arose (Gildas). Uncertainty and poverty prevailed; down-and-out humanity spoke with increasing awe and ignorance of the Good Old Times, and legend arose. Cut off from the easy mass-production of the Roman world, the Celtic or sub-Celtic population lacked at the same time the leisure and the comfort which might once more have elicited its native genius. Art and craft for a time lay moribund.

Then came the Saxon occupation. Teutonic farmers, obstinate but domesticated, began to settle in eastern and southeastern Britain, and to spread inland towards the midland plain. As villagers and farmers, these Teutons were essentially peaceful folk, and, after the initial conflict (renewed only as further territory was required) they settled down to their muttons. This Saxon Peace secured for the subject British population something of the comfort and leisure which, five centuries earlier, the Roman Peace had brought to their ancestors. At the same time the Saxon craftsmen, active though they were, did not so completely flood the country with mass-produced goods as the Romans had done before them.

Thus for the first time for over 300 years the Celtic craftsman, in the sixth century A.D. and under the Saxon régime, was freed alike from insecurity and from overwhelming competition. Under these circumstances, we might on general grounds expect some re-emergence of his art; and the Celtic escutcheons are the answer to our expectations. On the frontiers of Britain, uncemented by Saxon settlement, that social and spiritual unrest whereof Gildas is a spokesman prevented any similar artistic regeneration until the Celtic Church, disciplined and consolidated by contact with the new outposts of Roman orthodoxy in England, and enriched by a far-famed proficiency in learning, was able to offer to the native artist a greater degree of congenial patronage and protection than was available amongst the yeomanry of England. Accordingly, in the latter half of the seventh century the centre of (Celtic) artistic interest shifts from Saxon England to the Celtic Church.

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On this line of thought, the whole sequence of Celtic art, in my view, becomes as clear and logical as such subtle equations between temperament and opportunity are likely ever to be. On four occasions the Celtic artist in Britain found himself in that sympathetic environment without which his peculiarly sensitive and individualistic genius was unable to fulfil itself: first, during the Belgic and earliest Roman occupation of southern Britain, A.D. 1-60; secondly, during the earlier Roman occupation of northern Britain, in the first half of the second century A.D.; thirdly, during the Saxon occupation of central and southeastern Britain in the sixth and seventh centuries A.D.; and fourthly, during the régime of a strong and wealthy Church in Scotland and Ireland during the seventh to ninth centuries. In each instance, the essential circumstances were similar in kind, though they varied in degree. During a period of a millennium, an implicit Celtic art found expression, with numerous variations but an essential oneness, whenever and wherever the Celtic artist was given security and patronage without devastating rivalry; that is to say, where the economic conditions appropriate to his sensitive and almost feminine temperament supervened. So far as history is concerned, the basis of the problem is, I would urge, economic rather than aesthetic, and the proper perspective of the problem has been distorted, more often than not, through an over-emphasis of its artistic and typological aspects.



FIG. 3
Bronze triskele from a 4th-century stratum
at Verulamium, 1931. $\frac{1}{4}$



FIG. 4
Celtic escutcheon (6th-7th century type), of bronze,
enamelled in red and yellow. Possibly found at Oxford.
Pitt-Rivers Museum, Farnham. $\frac{1}{4}$

German fortified churches in Transylvania

by E. G. SEBASTIAN

FROM very ancient times the church and the churchyard have afforded refuge to villagers in time of war. For this reason, wherever practicable, churches have been built on heights, to be the more easily defended. The church served not only as a house of prayer but also as a protecting citadel, defending the lives and property of its children. Already in the 4th century the Armenians had made strong citadels of their churches. The Franks in Merovingian times (481-751) built fortified churches of which that of St. Jean at Poitiers still stands, as well as the church at Remainmontier. After the Saracen invasion most of the churches in the south of France were surrounded with defence works, whereas in northern France they were not defended before the English wars in the fourteenth century. In the Middle Ages most of the churches in the strip of land between the Rhine and the Nahe, called the Gau, were fortified. Osthofen had defence works as early as 1241. In the Middle Ages, too, fortified churches were built in Alsace and Lorraine, or else the existing ones were greatly strengthened. An especially characteristic example is Chazelles in Lorraine, built in the 12th century, in which we are first struck by the placing of the church-tower between the choir and the nave and then by the loop-holes and machicolations.

In the districts of Werra, Römhild, and Heldburg and in all the neighbouring country of lower Franconia there are fortified churches which were probably built towards the close of the Middle Ages. They were also built in Bavaria, of which Köffler affords a good example, and in Krain, west Styria and lower Austria, where some of the churches are in a good state of preservation, such as St. Oswald at Eisenerz. When the Turks threatened invasions in 1603 and 1683 the churches in lower Austria were made as strong as possible, but they were unable to resist the attack and the defenders were put to the sword.

Much more thorough in construction are the fortified churches in Transylvania, distinguished by the variety and strength of their defence works and by their conversion into veritable strongholds.

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Though one is accustomed to think of the church as a home of peace and quiet devotion, in Transylvania, the borderland then between east and west, it was above all for centuries a bulwark against the horrors of war and streams of blood have flowed around its walls. These churches, of which there were in the 16th century as many as 300, had to shelter the inhabitants of the community and often for long periods of siege; consequently they had to be completely self-contained and in all of them there was a well, and in some, such as Tartlau, a mill for grinding corn. They had to be built for this specific purpose, rather than hasty adaptations of former buildings. With this end in view their constructors triumphantly solved the problem of combining a church with a fortress. These churches are for the most part unpretentious works of art, but they are witnesses to times full of great anxiety and of bloody deeds. Few monuments of art can express so fully and peculiarly the spirit of a stormy, troubled period. As proofs of former struggles in Transylvania they acquire a very real historical importance.

The oldest Transylvanian fortified churches date from the 13th century. In 1241 the first Mongolian invasion laid Transylvania waste and the German colonists had to take steps to protect themselves against these wild hordes. They chose as the centre of their village life the church and they fortified it as well as they knew how. When, however, the Turks pressed forward into Europe and invaded Hungary in 1391 and captured Kronstadt in 1421, the fortifications were found to be insufficient. At that time practically every German village built its own fortified church or strengthened the existing building. It was necessity which called into being these remarkable buildings, 'whose aspect', as a German writer says, 'at first leaves one in doubt as to whether one stands before a House of God or a fortress of unusual construction, churches in which the neglect of the exterior is sometimes carried so far as to abandon altogether ecclesiastical characteristics, so that one must enter to convince oneself of the religious purpose of the building'.

Most of these buildings were constructed between about 1450 and the beginning of the 16th century. There were, however, already churches on heights and surrounded with walls earlier than this, for in 1436 the Dean of Hermannstadt reported to Pope Eugenius IV that the Germans, when pressed by the Turks, took refuge in the fortified courts of their churches and that the church-towers, even the churches themselves, served as a bulwark against the attacks of the enemy.

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The Transylvanian fortified churches belonging to the German villages may be divided into two main groups according to their construction. To the first group belong those in which the church itself has no defensive works, but relies entirely on the massive walls with their strong towers and bastions which surround them. Of these the majority are in the Burzenland, the plain round Kronstadt where the passes through the Carpathian Mountains to Kronstadt and to Törzburg open out into the plain to the north. Of these the most complete are Tartlau, which is described in detail later, Honigberg and Neustadt bei Kronstadt. In some of these it is true that the church-towers are provided with wooden galleries but they were more in the nature of watch-towers than for defence.

To the second and by far the larger group belong those churches in which in addition to having strong defensive walls the churches themselves are fortified, that is to say the actual church building. Of this group there are many types and we will take as type 1, those in which there is a remarkable style of defence, namely a loop-holed defensive gallery running round the church, the wall of which is borne on arches resting on buttresses; the whole wall is pushed forward to its own breadth so that between it and the church wall proper there is room for wide machicolations, which could be used for pouring pitch on the attackers, who came up to batter down the walls. A splendid example is Scharosch bei Gross Schenk, shown in plate 1. Otto Piper, in his book on fortifications, mentions that this style of machicolation is peculiar to Transylvania. 'Beyond Transylvania', he says, 'I know of only one example of this style of machicolation, that is the church of St. Peter in der Au in lower Austria'. Of this type there are three distinct sub-divisions :—

Type 1a. The machicolated arches surround the whole building and the height of the church is uniform on the outside, there being no difference between the height of the choir and the nave. Examples of this are Scharosch bei Gross Schenk, Jakobsdorf, Keisd and Klosdorf.

Type 1b. The machicolated arches surround the choir only, which is generally built up higher than the nave. In all other respects the fortifications are the same. Examples are Bonnesdorf—this church is slightly peculiar, for the arches, which form the machicolations, are further supported each by a corbel between the buttresses; Agnetheln—this was originally so defended but the defence works have been lately

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removed ; Bussd bei Mediasch—here the choir has been built up very high but the defence works are the same ; and Gross Kopisch, Gross Schenk, Meschen, Pretai, Roseln, Stein, and Trappold.

Type 1c. Here only the nave of the church is defended in this peculiar way. Examples are Bodendorf, Radeln and Wurmloch—in the case of the last the nave is defended by the usual arches carried on buttresses, but in addition the choir has been built up very high indeed and also has arches borne on buttresses. I have not, however, included this church in type 1a, since the nave and the choir are not of the same height.

Variations of the first two of these types exist, in which the machicolations are formed by arches resting on widely-spaced buttresses, in between which are corbels also helping to bear the weight of the arches. A good example of this variation in type 1a is Bussd bei Reussmarkt, and of type 1b Klein Schelken shown in plate II. The high built-up choir can be seen on the right. On the side facing there are four buttresses and between each pair are four corbels, all of which help to bear the weight of the gallery above and leave space for machicolations.

Type II. In this type the choir has been built up high, often into a massive tower, but there are no arches built out on buttresses. It is surrounded as a rule by a stone or wooden gallery, as can be seen to right of plate III, which is of the church at Eibesdorf. As can be seen most of the loop-holes have now been filled in. The best examples are Almen, Bulkesch, Eibesdorf, Heltau and Hetzeldorf.

Type III. In this type the height of the choir and the nave are the same and they carry no arches. Generally only the choir is surrounded with a stone or wooden gallery with loop-holes. In the case of the wooden galleries they are generally built out sufficiently far to allow of the pouring down of pitch. This type can be divided into (a) those with stone galleries : Baasen, BIRTHÄLM, Dobring, Frauendorf, Galt, Grossau, Katzendorf, Margarei, Neithausen, and Waldhütten ; (b) those with wooden galleries, or galleries which have been built up with brick and plastered over as in the case of Klein Schenk in plate IV, the best examples of which are Deutsch Weisskirch, Draas, Henndorf, Meschendorf, Proptsdorf, Schönberg and Schweischer.

As typical examples of these fortified churches I will describe two of the principal ones, BIRTHÄLM (plates V, VI), and Tartlau (plates VII, VIII), for almost all the important features which occur in the others are to be met with in these two.

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BIRTHÄLM is delightfully situated in a wooded valley ten kilometres from Scharosch bei Mediasch, which lies on the main road from Schässburg to Mediasch, that is to say in the valley of the river now known as the Tarnava Mare, its Roumanian name, in what is called the 'Weinland'. The district was settled by the Germans about 1150, and this particular group came from Luxemburg. The first church, as is general throughout Transylvania, was Romanesque in

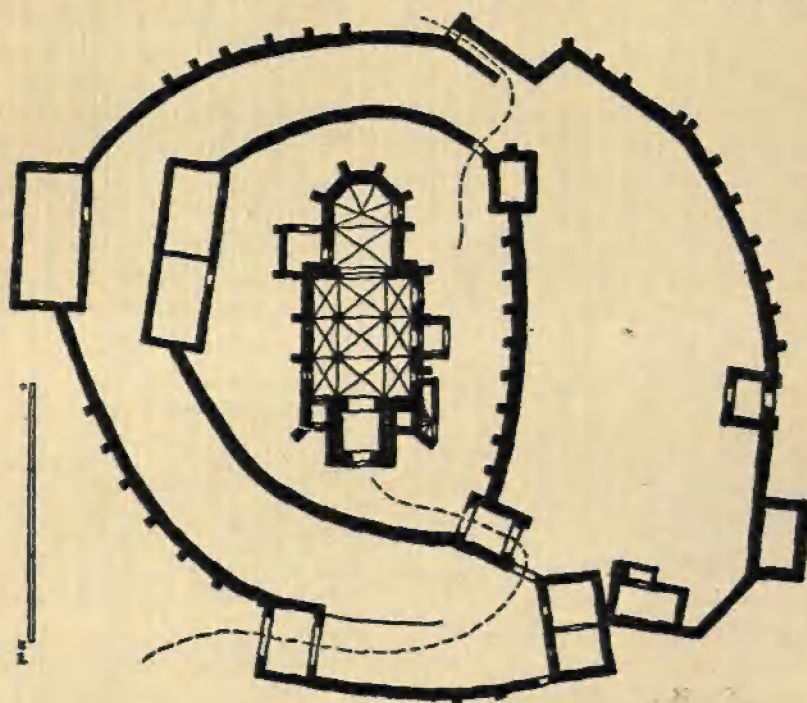


FIG. 1. TRAPPOLD

style and was probably partly defended. The present church was built in Gothic style between 1480 and 1524. The ground plan was taken from that of the upper church at Schässburg, which was completed just before. It is very imposingly situated on a hill some sixty feet high in the centre of the village.

The plan of the defences somewhat resembles that of the church at Trappold (fig. 1). It was originally surrounded with four walls, the lowest of which has disappeared to make room for the road which

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now runs around the foot of the hill, though some traces of it may be seen in certain of the houses there. The other three are intact except for the fact that they were originally considerably higher and provided with loop-holes above the machicolation. The centre and the upper of the existing walls are supported on buttresses which carry arches forming wide machicolations, the space down which the pitch could be poured being as much as a foot across. The original approach to the church passed through a strong tower in the middle wall and then up a narrow passage-way between this and the innermost wall until it reached a point near the top, when it passed underneath a further tower, beyond which lay the church enclosure. In plate VI this passage-way is shown looking down from the tower in the innermost wall to the tower in the middle wall, the height of the walls and the narrowness of the passage making it exceedingly difficult for the attackers to fight their way up. In these walls there were originally five strong towers and seven bastions at strategic points, but of these only three now remain. The towers were originally defended with loop-holes and with galleries of wooden planking, which, as well as providing protection from which to shoot, formed what amounted to machicolation. Plate V shows on the right the tower at the southeast corner of the church enclosure with its wooden gallery. Around the choir runs a stone gallery pierced with loop-holes, but the main defence lay in the exceptionally strong system of walls and towers and in the height of the hill on which it stands. The original inhabitants chose their site well and then proceeded to add to its natural advantages this ingenious system of defence.

Just within the innermost wall on the hill-top to the south of the church is a large square tower, a survival of the original church, known as the 'Catholic Tower'. In the ground floor of this tower are several frescoes dating from the 15th century which still retain their original vivid colours, although they have been very badly treated one way and another. The subjects are as follows :—

Over the east window Our Lord with angels in red-gold garments and wings of the same colour. St. George in iron armour and spiked helmet with a shirt of mail under his armour. He has a cross-hilted sword in a red leather baldric decorated with gold and is attacking the dragon, holding in both hands a lance with fringed point. On the side opposite is a full-length figure of the Archangel Gabriel.

On the south wall to the left is the Annunciation and to the right the Visit of the Magi. Between them is painted a vivid green and

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black cloth against which the altar originally stood. There is also an inscription, now illegible, in the new Gothic small writing of the 15th century.

On the west wall are many figures in bright coloured garments, some having red hats like cardinals, some simple white kerchiefs, to whom an angel is speaking; a saint worshipping, Christ on the earth globe, a bishop with a crozier and a man with a raised sword.

On the north wall is a female saint in a white garment, a man beside her aiming with a bow at her heart. Over the door a curtain beneath which are the weapons of torture, cross, scourge and whip. On the other side of the door an arrow flies at a fair-haired woman in an ample cloak, before whom several persons kneel, among them a bishop, while an angel with fair curls stands in front of her to defend her from the arrow. On the vaulted roof is a large figure of Christ; the right hand raised in blessing, the left holding the open book of the gospel. Above the door on the outside the Christ-child seated on Mary's knee is placing a crown on the head of a kneeling figure, while a king in iron armour and large mantle stands at the side, his left hand raised, his right hand swinging an elaborate battle-axe.

The coming of the Reformation to Transylvania was the work of one man, Johann Honterus, a citizen of Kronstadt, who was born in 1498. In 1553 a Lutheran bishop was appointed, having his seat at Laibach. In 1572 the third bishop appointed was the pastor of BIRTHÄLM and the seat of the bishopric was transferred there from Laibach and remained so until 1867, when it was once more moved, this time to Hermannstadt. There were 26 of these bishops at BIRTHÄLM and the majority of them were buried in the choir of the church. Of their carved tombstones seven are now to be seen in a tower to the southeast of the church, which stands to the right of the choir in plate v. Of these seven, five have been removed from the choir and two from near the church door.

The interior of the church consists of a central nave with aisles on each side, separated by three octagonal pillars, which support a finely proportioned Gothic vaulted roof. At the east end stands the choir slightly raised with a vaulted roof supported on arches which spring from corbels in the wall, there being no supporting pillars. At the east end of the choir are three lancet windows, each with three lights with good Gothic moulding. The altar-piece is a central panel with two wings, the centre of which consists of a carved wooden crucifix with figures on either side. Around it are ten panels with scenes from

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the life of Our Lord and the two wings contain a further eight panels with other similar subjects. It was completed at the same time as the church. The open-work Gothic wood-carving over it is particularly fine. The choir-stalls are of carved and painted lime-wood, typical of these churches, although now many of them have disappeared, in the backs of which are inlaid panels of marqueterie. They were probably made by one of the sons of Veit Stass of Nuremburg, who lived at Mediasch. Good specimens of this style of choir-stall are to be seen in the upper church at Schässburg and at Bistritz. The pulpit is of carved stone, contemporary with the church. The door leading to the sacristy is of great interest and bears the date 1515 and the initials of the pastor. The lock is extremely complicated and is composed of thirteen bolts covering some four square feet ; the iron ornamentation on the lock and handle are perfect specimens of Gothic iron-work.

TARTLAU is a small town some 20 kilometres northeast of Kronstadt and possesses the largest of the fortified churches in Transylvania. The town stands well away from the hills in the centre of the plain of Kronstadt, the 'Burzenland', as it is called. This was formerly lake and marshland, in fact the pastor told me that his father used to talk of how he habitually fished in the lakes near the town, which have now all disappeared. Consequently the natural defences of the place rested in the water surrounding it and not, as at Birthälm, in the height of the hill on which it stood.

Standing as it does in the plain it has been necessary to surround it with very strong, high walls. The ground-plan of the church (fig. 2) shows that the main wall surrounds the building in an almost perfect circle. This wall is very high, standing up over 40 feet, and is provided with pitch-spouts for pouring pitch on the attackers ; around it also ran a wooden gallery with loop-holes for marksmen. In it there were originally four strong towers, but of these only two remain. From this main wall juts out the main entrance to the church. The approach is very strongly guarded and access can only be gained through a narrow passage-way, which passed originally over a draw-bridge across the moat, which surrounded the entire building. The end of this passage-way debouched on to a strongly-fortified forecourt (plate VII), from which one could only reach the innermost court and the church itself through a further low passage, which could be closed by means of a heavy portcullis, which is still in position. On the left of this forecourt is a small triangular court, also strongly defended, which was known as

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the 'Baker's Yard'. Against the inner face of the main wall in the church-yard, as was almost always the case, living-rooms and store-rooms have been built (plate VIII), in three tiers, and under them are cellars. Rooms have also been built around the forecourt, and in the 'Baker's Yard'. In these cellars were stored provisions enough to last through a protracted siege and the arms and ammunition were stored in the towers in the main wall. As Tartlau lies not far from the

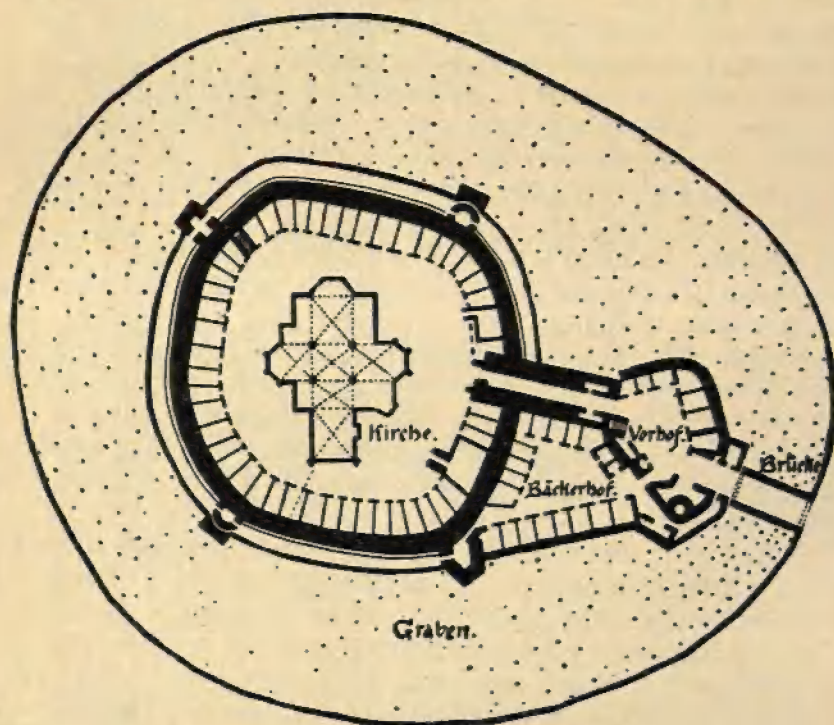


FIG. 2. TARTLAU

mouth of the pass, which leads over the Carpathians to Kronstadt, it was the scene of constant raids by the Turks, Wallachians and Moldavians. When the inhabitants were warned by their scouts of the approach of the enemy, they withdrew within their church, taking with them their cattle and what they held most precious. These they stored in the living-rooms and store-rooms allotted to them. Inside the courtyard is a well and there was also a horse-driven mill to grind the corn for the bakers to enable them to be self-supporting during

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a prolonged siege. Besieged, too, it was many times, notably in 1552 by the Voivodes of Moldavia and again in 1658 by the Turks. It was once more besieged in 1704 during the Kurutzen disturbances, as was Birthälm. The fact that it has never been captured bears eloquent witness to its strength, and the stout hearts of its defenders.

There is nothing of interest in the interior of the church, which is very simple in construction and in no way comparable to the church at Birthälm.

NOTES ON THE PRINCIPAL CHURCHES

AGNETHELN (AGNITA). The town lies at cross-roads in the centre of the triangle formed by Kronstadt, Schässburg and Hermannstadt. The fortifications were removed in 1892, but there is a fine church-tower with bricked-in gallery and two of the towers in the walls remain. The church is mentioned as early as 1466 when King Matthias decreed that, when a general muster of all those capable of bearing arms was called, half of the citizens of this town might remain behind to defend it, so important was its position held to be.

ALMEN (ALMA). The choir rises above the nave and the upper part of the wall has been built out on corbels, forming wide machicolations, there being a loop-holed gallery above. The nave appears to have had no defensive works. A high wall surrounds the building in which four of the original towers still stand. It originally had loop-holes but most of them have been filled in. A gallery, too, on the inner side, has disappeared. The eastern tower in the wall is in good state and appears to have been very strong, having a wooden gallery surrounding it. The loop-holes are divided into three partitions each to enable defenders to shoot to right and left as well as straight in front without moving position.

ALZEN (ALTINA). A single wall, now in rather a dilapidated condition, surrounds the building, but many parts have been pulled down. Some original towers in the walls still stand, but they are of no importance. The church has been disastrously restored and there are now no traces of any defence works.

ARKEDEN (ARCHITA). Surrounded with a double wall which is loop-holed and defended with seven strong towers, one of which still has its bricked-in gallery. The other towers lost their galleries in the great fire of 28 August 1748, which did much damage to the building. One tower is dated 1661.

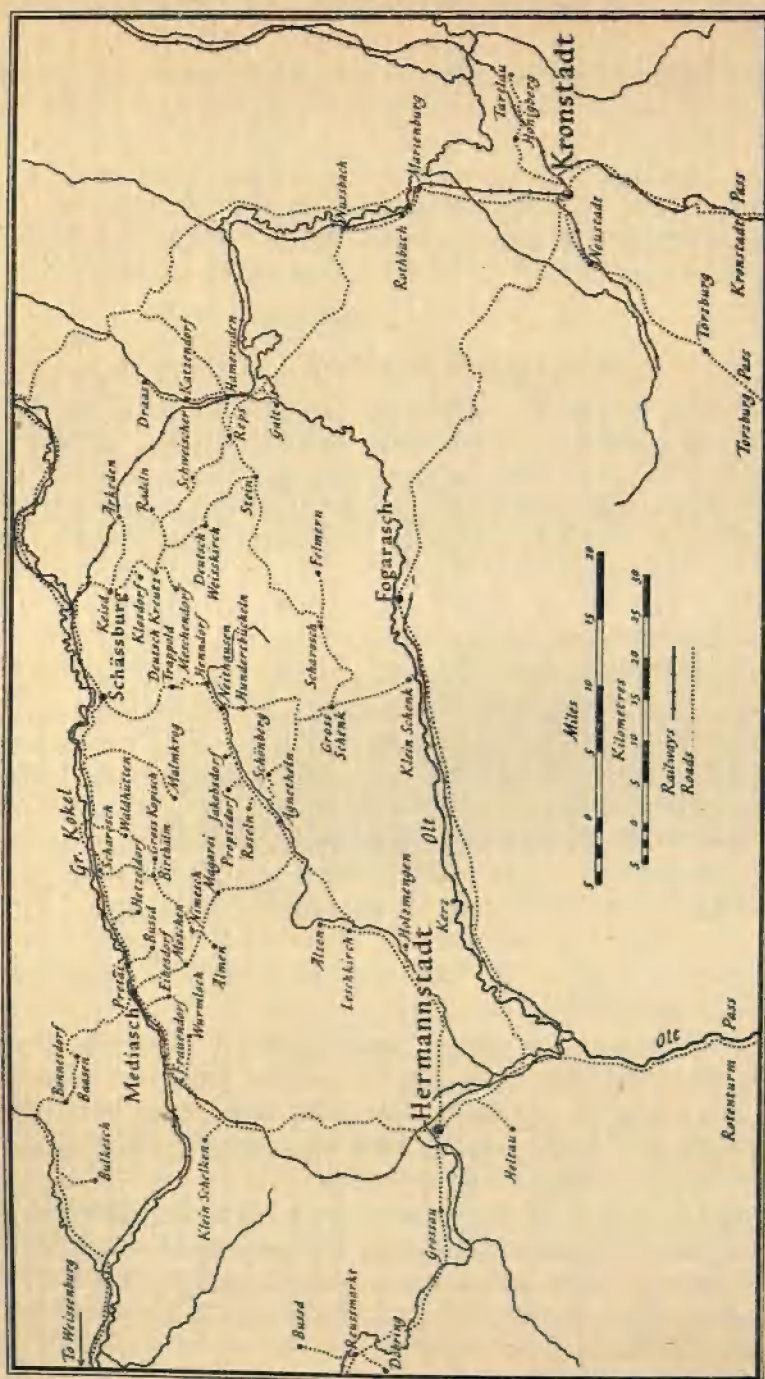


FIG. 2. TRANSYLVANIA

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BAASEN (BASNA). The nave is quite undefended but the choir is built up into a massive tower supported by buttresses at the corners and in the centre of the sides, but these never carried machicolations, they were only for the support of the fabric. The gallery which surrounded it at the top has disappeared but there are two rows of loop-holes in the lower part of the tower. The church in its present state was completed in 1504.

BODENDORF (BUNDORF). The fortified church-tower has been removed but the nave is machicolated. The choir is loop-holed and slightly lower than the nave. Below these loop-holes are spouts for pouring pitch. Built in 1519.

BONNESDORF (BOIAN). The nave has no defence works but the choir has been built up into a massive tower, which is reinforced by buttresses at the corners and in the centre of the sides, as at Baasen, but in this case machicolated arches have been built between, resting on corbels. Above is a loop-holed gallery. The gate-tower in the wall is in good state. Completed in 1506.

BULKESCH (BALCACIU). The choir rises above the nave and is fortified with a wooden gallery, panelled man-high, below which it is loop-holed. The building is surrounded with a wall with small towers in it which have been much restored.

BUSSD BEI MEDIASCH (BUZD). The choir is built up in three storeys, very strong with a winding staircase in a circular turret to the side. Fortified with buttresses forming wide machicolations and each of the storeys is pierced with loop-holes. Built 1490-1495.

BUSSD BEI REUSSMARKT (BOZ). Choir and nave are of the same height and are supported by buttresses, between each pair of which are three corbels carrying a built-out gallery and forming machicolations below. A turret with a winding stair leads to this gallery.

DEUTSCH KREUTZ (CRIT). Has been restored but much of the original wall still stands with bastions, and machicolations on corbels.

DEUTSCH WEISSKIRCH (VISCRI). The outer wall is exceptionally strong and high and the entrance in it is covered by a strong tower, with wooden gallery, and loop-holed. There are several other towers and bastions with wooden galleries. The ground-plan is an oval, 46 yards by 36. The choir was originally machicolated but only traces now remain. The north tower bears the date 1630 and the west tower 1649, but these were added later than the rest of the fortification.

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Around the inner wall are still to be seen the store-rooms and living-rooms, which were occupied in times of siege.

DOBRING (DOBAREA). The wall is in a very bad condition but in front of the entrance there is a small forecourt, over the entrance to which are still in place two pitch-spouts for pouring pitch on the attackers. Inside this entrance is the gateway of the inner wall. The church-tower covers the original hill watch-tower and rises in four storeys, each pierced with loop-holes. The tower was originally surrounded with a strong gallery with machicolations but the new church was built over and to the east of it in 1481. Inside can be seen, over the organ, the original corbels forming the machicolations, which are now covered by the roof of the church. In 1509 it was dismantled by Michael, Voivode of Wallachia, and was not rebuilt until 1631. Some of the original store-houses remain inside the church courtyard.

DRAAS (DRAOS). The church (plate ix) dates from very early times. The towers in the wall originally had galleries round them similar to that around the church-tower. The wall is loop-holed at intervals. Much of the first Romanesque church remains and the rounded arches are to be seen, built into the side walls. It was formerly much larger and had an aisle on either side, separated by these Romanesque arches, but it was re-modelled in the 15th century on a much smaller scale, the arches of the aisles becoming part of the outside wall. Above these arches are rounded windows each containing two semicircular lights. Under these windows are remains of early frescoes which would appear to be of some merit. The Romanesque west door is original. The Germans came to Draas in 1224 and are said to have brought with them a long double-handed sword, which is still kept in the church. The story goes that there were originally two of them, the other one being at Broos, not far from Hermannstadt, but that it disappeared in the beginning of the 16th century during one of the Turkish invasions. This one at Draas has every appearance of being authentic. The choir is surrounded with a bricked-in gallery, forming machicolations below.

EIBESDORF (IBISDORFUL SASESC). The church is defended on one side (plate iii) by a stream, and opposite to the crossing of the stream is a round bastion, the only round one which I have seen in these churches, provided with loop-holes and pitch-spouts. The walls are in a state of decay but the choir is built up high and strongly loop-holed and the church-tower is surrounded with a wooden gallery.

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There is good Gothic work in the interior and an aumbry dated 1421. A small staircase leads up in the thickness of the wall to the gallery above. Some of the store-rooms are still in place.

FELMERN (FELMER). Originally built in the Romanesque style of which traces remain. Portions of the original wall and some of the towers are intact. The galleries of the towers have been removed and the roofs now rest on the walls themselves. There are no traces of fortifications.

FRAUENDORF (FRÂUA). Begun in 1330; the fortifications were strengthened at the end of the 15th century. The choir was heightened and loop-holed and a bricked-in gallery was built round the church-tower. At this time a high wall with towers in it was built round the building, which is in an almost perfect condition. The west door is a fine specimen of Transylvanian Gothic work. The name is derived from the 'Frauen' = nuns, since there was once a nunnery there.

GALT (UNGRA). One of the oldest of the Transylvanian churches and a certain amount of Romanesque architecture still survives, notably the arches, which formerly cut off the north aisle but now are built into the outside wall, as at Draas. The church was captured by the Turks in 1658. It was rebuilt in 1702. There is a circular apse of basilican type at the back of the altar at the east end.

GROSSAU (CHRISTIAN NEAR SIBIU). The village lies close to Hermannstadt near the mouth of the Rotenturm Pass through the Carpathians by way of the Olt Valley and a favourite one for the Turks to take. Its defences were strong—a surrounding wall, which was double with towers and bastions, and formerly had a loop-holed gallery and was machicolated. The galleries have been removed from the remaining walls but it is obvious that they were strong. The date for the original church would be about the middle of the 12th century. This church was burnt by the Turks in 1493, and the present church built in 1498. Grossau appears to have been a Christian settlement even before the advent of the Germans and its original name was 'Insula Christiana'.

GROSS KOPISCH (COPSA MARE). The church was originally Romanesque, but was fortified towards the end of the 15th century, when the choir was built up high with its characteristic machicolated arches and stone, loop-holed gallery. There is only a single wall around it.

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GROSS SCHENK (CINCUL MARE). The town is at an important centre in the German country not far from Agnetheln, and the church was originally Romanesque. It was fortified and transformed into Gothic in 1522, when it was surrounded with a double wall with several towers, but of these nothing remains. The church-tower is massive and appears to have been built over the remains of the former church. The name is derived from Schink, a name found in Luxemburg.

HAMERUDEN (HOMOROD). Built between two massive towers, the easternmost of which stands over a basilican chapel of the earlier church, dating from the 13th century, as can be seen from the arching of the roof, which has been cut into by the wall of the present church. Consequently the altar stands on the south side. There are some good frescoes of possibly 14th century, but they are sadly damaged. The chapel is rounded off with a semicircular apse with semicircular roofing. Over it was built the tower by adding some six feet of building as a casing, in the depth of which, through one of the original windows, a staircase winds up to the upper storeys. The other window in the apse has been blocked in but can clearly be traced. This superimposition of a tower over an earlier chapel or portion of the original church is often to be found and is in fact almost always the case when there is a very wide church-tower. Notable examples are Scharosch bei Gross Schenk, Neithausen, Roseln and Radeln. The defences lay in these two strong towers with their wooden galleries and in the double wall, though the outer one has almost disappeared. There are two towers and a bastion in the wall in fair condition. This wall was originally loop-holed and was surrounded on the inner side with a wooden gallery (plate x).

HELTAU (CISNADIE). Originally built in the 12th century, and was Romanesque. The arches can still be seen. Destroyed by the Turks in 1493 but afterwards rebuilt in Gothic style in the form of a cross, high superstructures, like towers, being built for defence over the choir and the north and south entrances. It was at the same time surrounded with a triple wall with pitch-spouts and between the inner and middle of them was a moat, which was only filled in in 1880. Inside the gateway is a small forecourt very strongly defended. Of the many towers and bastions which formerly stood in the walls only three remain, one of which was originally a chapel but has now been converted into a bacon store. This is a common practice, and use is made

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of these towers on account of their coolness and even temperature. Around the inner side of the wall, as at Hameruden, were store-rooms and living-rooms supported on brick arches, under which were cellars, but these have now been removed. The name was brought by the inhabitants from Germany. Sixteen gilt vessels from this church are now in the Brukenthal Museum at Hermannstadt.

HENNDORF (HENDORF). The choir and the nave, although of differing heights, are surrounded by a completely filled in wooden gallery with loop-holes cut in it. Not many of these churches have this type of gallery. The protecting wall was originally very high, so high in fact that the windows could not be seen from the outside. Only bits of it now remain. In the nave is a deep well which gives very good water. In almost all cases there is a well in the courtyard, but it is rare to find one in the church itself. Completed in its fortified state in 1465, but there must have been an older church. The name is derived from 'Hain' = grove, meaning the village in the grove. During the 17th century the Turks captured it and pulled down the church-tower.

HETZELDORF (ATEL). There are good trefoil windows in the church-tower, which are now unfortunately blocked up. When the church was fortified another storey was added to this tower and a panelled gallery built under the roof. The choir, too, originally had a gallery but this has been removed. Some of the towers in the wall are still standing, but the wall itself is considerably lower than formerly.

HOLZMENGEN (HOZMAN). There are traces of the original Romanesque church, and the doorway of the west tower is pure Romanesque. This tower was not enlarged when the church was further fortified, but a wooden gallery was added, panelled right up to the top with loop-holes cut in it. The original wall still stands with towers in it, but these have all lost their galleries with the exception of the gate-tower, which is machicolated, and has a portcullis of iron-shod oak.

HONIGBERG (HERMAN). Surrounded with a very strong wall 40 feet high and provided with pitch-spouts. There are 7 strong towers in a good state of preservation, but the galleries have been removed and the roofs lowered on to the walls themselves. The main tower originally had a wooden gallery, but it was destroyed in 1794. As at Tartlau a broad moat ran round the building outside the wall, but this has now been filled in. A drawbridge crossed the moat forming a pillared passage-way as at Tartlau and at the end of this passage was

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a dark, vaulted corridor, which could be closed by means of a portcullis, still in position. This corridor leads into the courtyard of the church, where there are several living-rooms and store-rooms. Further rooms were built against the church itself, between the buttresses. In the courtyard is a well and there were previously a bakery and a horse-driven mill. The church is said to have been begun during the 13th century and was burnt down in 1593. It was rebuilt in 1595. It sustained a prolonged siege by Gabriel Bathory in 1612 and again by the Turks in 1658. Charles XII of Sweden visited it while in this part of Europe and took such a fancy to it that he gave the inhabitants money to build a new altar.

HUNDERTBÜCHELN (HUNDRUBECHIU). The building is very narrow and is wedged between two high towers. The westernmost of these still has its wooden gallery panelled right up to the top with loop-holes cut in it, as at Henndorf.

JAKOBSDORF BEI AGNETHELN (JACOBENI). As at Hameruden, the main tower has been built over a portion of the former church. It is very strong and heavily machicolated and is joined to the square nave on the west side. Choir and nave are both surrounded with arches, which bear a loop-holed stone gallery and form wide machicolations, and the north wall of the choir is joined to a further tower, which has a loop-holed wooden gallery. Surrounded with a wall in a fair state of preservation and in it one tower with its gallery bricked-in man-high.

KATZENDORF (CATA). The double wall is in good condition and in it are three towers. One of them has a wooden gallery, one is bricked-in and the third, which rises over the original entrance in the inner wall, had a stone gallery, but now only the corbels remain.

KEISD (SASCHIZ). A good specimen of type 1a, with the surrounding arches and machicolation. It was built between 1493 and 1496. There is now no trace of the wall left. In front of the church stands the bell-tower, which stood within the original wall. The upper structure of the old tower was burnt down in 1714 and was rebuilt in 1718 as an exact copy of the clock-tower at Schässburg.

KLEIN SCHELKEN (SEICA MICA). The church has some peculiar features. Originally surrounded with a double wall with several towers in it but now only three remain. Around this wall on the inner side ran a covered gallery resting on stone arches. The choir rises high above the nave (plate II). It is machicolated by means of

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corbels, which jut out from the wall and bear above them the usual stone loop-holed gallery, there being further loop-holes between the buttresses which surround it. The tower which rises over the west door has a wooden panelled gallery, and outbuildings have been built against it on the north and south sides, which, too, are loop-holed and rise above the height of the nave, which is not fortified in any way. Between this tower and the new bell-tower, which stands to the west of it, lies a small courtyard surrounded with a very high wall, which has a gallery running round the inside borne on stout buttresses and over the small doorway leading into it is a pitch-spout. This little courtyard was the last place of defence for the inhabitants, and, as the walls are very thick and strong, was of great strength. On the south wall of it is a small watch-tower facing south with a bricked-in gallery. The church was built towards the end of the 14th and fortified early in the 15th century.

KLEIN SCHENK (CINCOSOR). A good specimen of type III, with a wooden gallery (plate IV). It is surrounded with two walls in which are six large towers and two smaller ones, all of which are provided with bricked-in galleries, in a splendid state of preservation. The entrance to the outer wall is on the south side and over the doorway is a pitch-spout. The church is defended by a bricked-in gallery, which runs round the choir, and the church-tower has a similar one. Within the walls is a well and there was formerly a mill. It was built in 1421.

KLOSDORF (CLOASTERF). Built in 1524. The nave and choir are the same height, both being surrounded with buttresses on which rest arches making wide machicolations. The usual loop-holed stone gallery surmounts the whole. A single wall of some height surrounds the whole building, but it is now devoid of towers. The old church-tower has been pulled down and a modern one built in its place. There is a well in the nave.

LESCHKIRCH (NOCHRICH). One of the earliest settlements of the Germans, but the church has been so renovated that it no longer contains anything of interest. Four small towers, which originally stood at the corners of the wall, are still standing.

MAGAREI (MAGAREI). On a fairly high hill and surrounded with a single wall, in which one slender tower still stands with bricked-in gallery. There are a few bastions, which jut out from the wall. On the west side, that is the side facing up the hill, the wall is provided with

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pitch-spouts. The church itself has no defence works and in fact can hardly be seen above the church wall.

MALMKROG (MALANCRAV). The wall has been cut down to a negligible height and there are no towers now standing in it. The church-tower rises to a considerable height and has a wooden gallery. The interest of the church lies in the frescoes. The whole of the choir (plate XII), including the roof, is painted with scenes from the life of Our Lord and of the four evangelists. They give a vivid impression of the artist's conception of the bible story and appear to be of the same date as the church, that is to say the 16th century. The north wall is covered with three lines of paintings of biblical scenes. The altar piece is a good specimen of 16th century painting and has two wings. It is unfortunately rather badly preserved. There is a charming little Gothic aumbry with a finely-worked iron grill of the same date as the church and above it is a very interesting stone crucifix, only some 18 inches in height, with a figure on either side, the whole being surmounted with Gothic canopy (on the left of plate XII).

MESCHEN (MOSNA). One of the best-known of the Transylvanian churches, its style being pure Gothic. There was an earlier church here but it was pulled down when the present one was built c. 1490-1500 by a certain Andreas Lapida, who had designed the church at Hermannstadt. The fortifications were at least in part constructed later, as can be seen from the date 1580 on one of the towers. The choir is machicolated in the usual way by arches resting on buttresses with a stone loop-holed gallery above. Strong towers rise over the north and south entrances provided with stone machicolation and a wooden gallery above. The defence wall, which is interrupted by four strong towers, is double in parts and has in addition two small turrets on the wall itself. The northern side of it consists of the north wall of the original church, the other portions of which were removed when the present one was built. On the inner side of the surrounding wall are store-rooms and living-rooms in two tiers and in addition against the southwest wall. These latter are more finished and were probably used as a dwelling for the priest and for schools in time of danger. The interior is very fine with splendid Gothic vaulting.

MESCHENDORF (MESINDORF). Built of golden sandstone and surrounded with a double wall in which are several strong towers. The one at the west end of the wall near the original entrance is particularly

PLATE I



SCHAROSCH BEI GROSS SCHENK

Showing group II, type Ia, with machicolation round choir and nave

Plates I-XII, *ph.* E. G. Sebastian

PLATE II



KLEIN SCHELKEN

Showing group II, type Ib, with the variation of corbels forming the machicolation

PLATE III



EIBESDORF

Showing group II, type II. No surrounding arches but choir built-up and loop-holed

PLATE IV



KLEIN SCHENK
Showing group II, type III. Choir and nave surrounded by bricked-in gallery, as also the towers

PLATE V



BIRTHÄLM: GENERAL VIEW

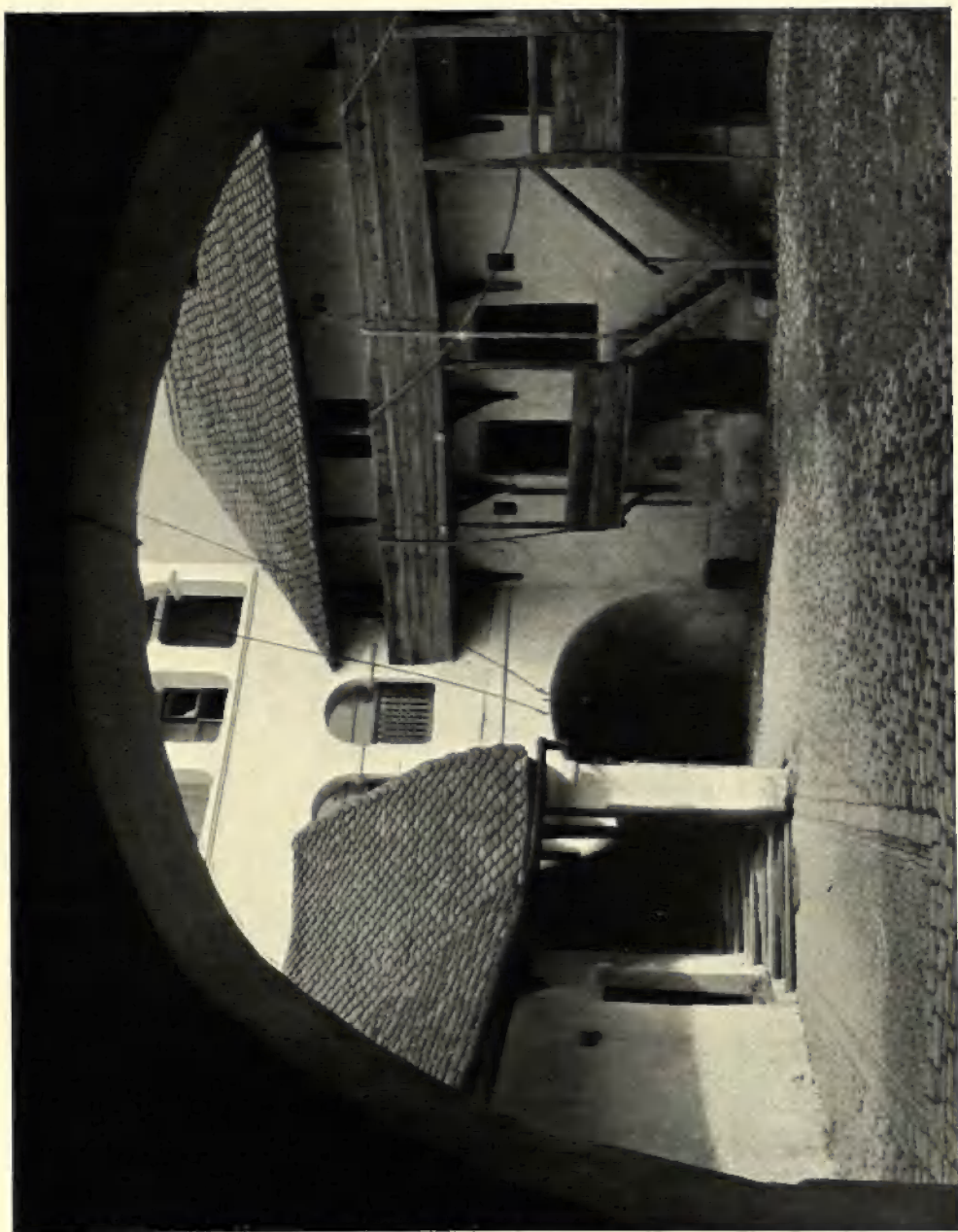
PLATE VI



BIRTHÄLM

Passage-way leading between the walls to church above

PLATE VII



TARTLAU

Forecourt, showing passage-way to church and store-rooms

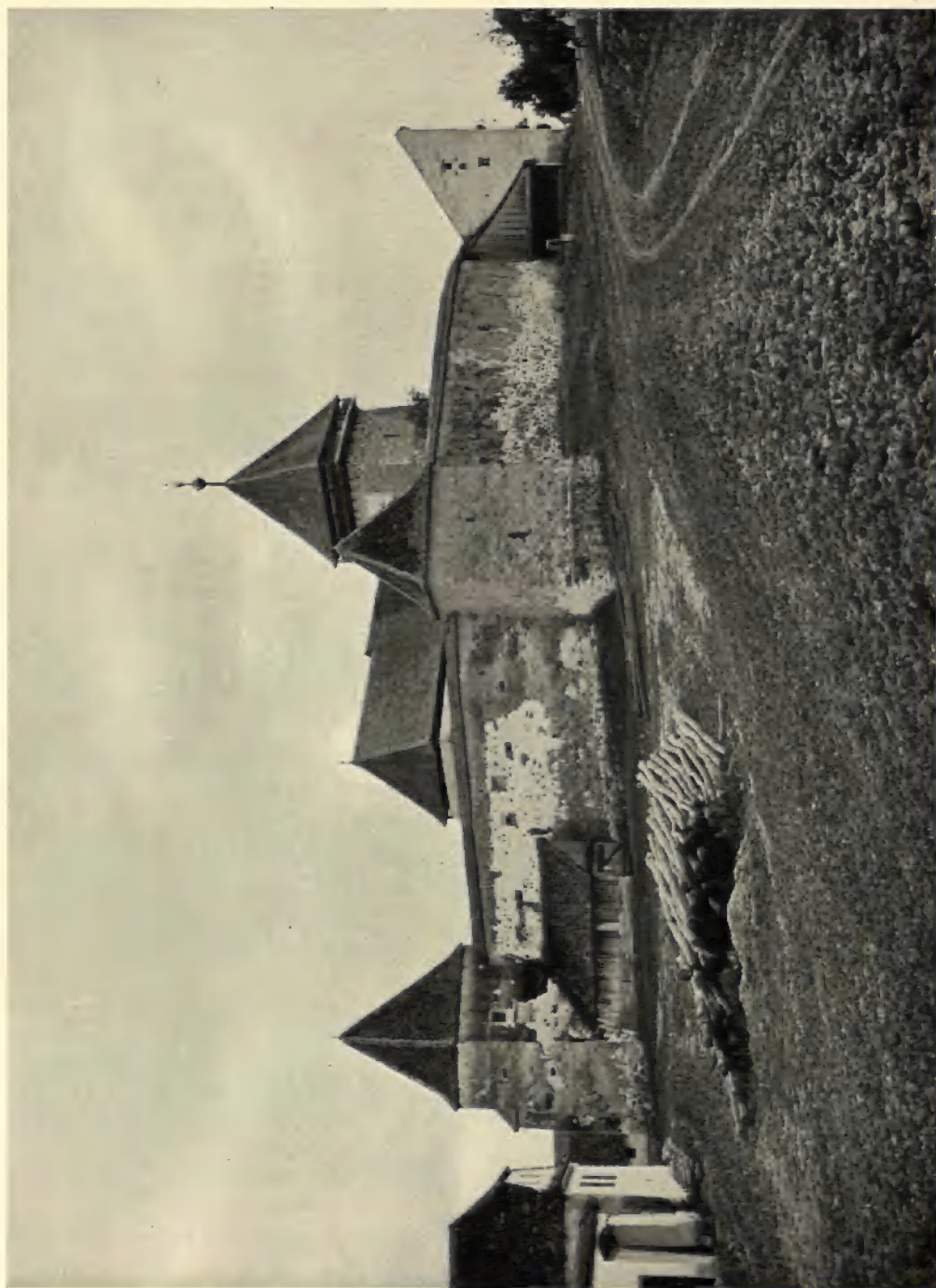
PLATE VIII



TARTLAU

Inner court with store-rooms and living-rooms in three tiers with cellars underneath

PLATE IX



DRAAS : GENERAL VIEW

PLATE X



HAMERUDEN

Inner courtyard showing remains of wooden gallery

PLATE XI



SCHÖNBERG

Courtyard showing living-rooms on left



MALMKROG
Showing frescoes and aumbry

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fine and has a bricked-in gallery. The church itself is surrounded under the roof with a wooden gallery, as also is the church-tower.

NEITHAUSEN (NETUS). There are the remains of a wall, cut very low. The choir and the nave were probably loop-holed with a gallery. The strong tower has been built over a portion of the original church, for at the back of the altar on the east end there is a small chapel with Gothic vaulting, which is no longer in use and which is obviously of much earlier date than the present church.

NEUSTADT BEI KRONSTADT (CRISTIAN NEAR BRASOV). This is one of the churches in the Burzenland, or the plain round Kronstadt, and consequently had a very strong wall, of which much still remains, and in which there are still nine defence towers standing. The church was entirely rebuilt in the 19th century. There is a small courtyard in front of the church which was originally defended. The wall is provided with pitch-spouts and some of them have been splayed out so as to enable them to be used for shooting to either side. Around the inner side of the wall is the usual gallery for the defenders. Facing the church on the inside of the walls were originally store-rooms and living-rooms, but they were removed in 1899. They were in two tiers and measured about 10 square yards.

NIMESCH (NEMSA). The defences have now all been done away with but the choir of the church is filled with a very interesting collection of frescoes of the 16th century, mostly of full-length saints. There are probably many more than are visible now as the plaster has not been removed but has only worn away in places.

NUSSBACH (MAERUS). The church lies in the Burzenland and consequently relies for its defence on its walls. There remains a single wall which is high and has alternate pitch-spouts and loop-holes in it. The church-tower has been newly-built and is of no interest. Part of the wall has been pulled down.

PRETAI (BRATEIU). The choir rises above the nave and is machicolated in the usual way with arches resting on buttresses. A strong bell-tower rises to the west of the church with a wooden panelled gallery. There is a good surrounding wall and the gate-tower is still standing and well-preserved. The west door is a good specimen of Transylvanian Gothic architecture.

PROPTSDORF (PROSTEA). The little church has great charm. Both the choir and the nave are of the same height and are surrounded

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with a bricked-in gallery. At the west end rises the church-tower, which has a wooden gallery. In the wall which surrounds it one tower still stands over the entrance.

RADELN (ROADES). The church stands on a considerable hill and is surrounded with a wall, double in parts, which was loop-holed and originally had five towers—now only four remain, one dated 1644. The nave is machicolated in the usual way with arches resting on buttresses with a stone gallery above. On the west side of the church rises a massive tower, which has been built over an earlier Gothic chapel with good vaulted roof and the remains of a good doorway, which is now sadly misused. Unfortunately the outside of this doorway has been blocked up with bricks. This tower is surrounded with a complete wooden gallery with loop-holes cut in it. In the choir is a good altarpiece with side wings dated 1614 and 1648. The date 1526 over the sacristy door shows the completion of the church as it now stands.

REUSSMARKT (MERCUREA). An important centre between Mühlbach and Hermannstadt. There is very little left of the church defences, the wall being still more or less intact and a small tower on the west end of the church, which has a wooden gallery.

ROTHBACH (ROTBAY). Lies in the Burzenland, or plain of Kronstadt, and consequently relies for its defence on its walls. There remains a single wall, which is high and has alternate pitch-spouts and loop-holes in it. The tower has been built over an earlier Romanesque chapel and at the top it has been built out on corbels forming machicolations. The gallery above has now disappeared. The east end dates from the original church and is basilican in shape.

ROSELN (RUJA). The choir is machicolated by means of arches borne on buttresses, but the nave is undefended. On the west side rises a very strong tower, which has been built over the remains of a Romanesque building and is surrounded with a complete wooden gallery in which loop-holes have been cut. Nothing now remains of the original defensive wall.

SCHAROSCH BEI GROSS SCHENK (SOARS). A perfect specimen of type 1a (plate 1). Choir and nave are both machicolated and there is a fine tower at the west end, which has been built over a vaulted chamber of the original church, having a Gothic doorway leading into the church. Built in 1486 and renovated in 1809. There are now no remains of its walls and towers, the last portions of which were removed a few years ago.

GERMAN FORTIFIED CHURCHES IN TRANSYLVANIA

SCHAROSCH BEI MEDIASCH (SAROSUL SAS). Originally surrounded with a single wall provided with loop-holes and pitch-spouts. Some of the towers still remain in a somewhat dilapidated condition but the gate-tower is fairly well preserved. Good Gothic windows in the choir and some good vaulting.

SCHÖNBERG (DEALUL FRUMOS). An interesting compact little church which was fortified in 1522. The three-sided choir was pulled down and a strong tower built over a square choir, which, as well as the tower on the west of the church, was machicolated and provided with a bricked-in gallery. The nave, which lies between these two towers, is low and has no defensive works but is protected as high as the roof by the church wall, which is very high. All these features can be seen in fig. 3. The galleries of the towers, and the gallery which runs round the wall, are of the type given in fig. 4, which shows the construction and is common to almost all the galleries in these churches. The original wall was strengthened later by the addition of a second wall on the north and south sides as shown in the plan. In these two walls are many towers still in a splendid state of preservation. Above the entrance to the church enclosure are pitch-spouts and there are others in the bastions on the walls. Around the inside of the walls are rooms (shown in plate XI).

SCHWEISCHER (FISERIU). Almost entirely renovated with disastrous results, there being practically nothing left of the fortifications, though a book on the subject quotes it as one of the most interesting of these churches. It was originally built about the middle of the 15th century but was captured and dismantled by the Turks. In 1605 it was once again fortified. The name is a Luxemburg place-name.

STEIN (STENA). The single wall which surrounds it still stands except at the southeast corner, where it has been removed to make room for school buildings. The wall is square, which is rather unusual, with towers at the four corners and a small bastion in the centre of the north side. The choir has been machicolated in the usual way with arches resting on buttresses with a stone loop-holed gallery above. The vaulting in the interior is good. The nave bears the date 1587, the completion of the church. There are good carved choir stalls in the style to be seen at Birthälm.

TARTELN (TOARCLA). The wall is very dilapidated and has been lowered to a negligible height but the tower is strongly built and covers the remains of an earlier church, the west door being in pure

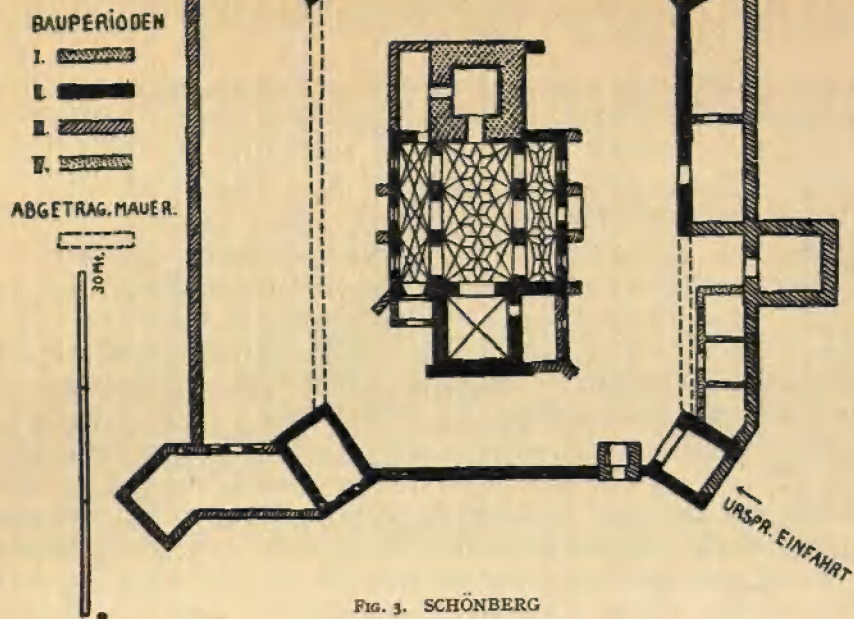


FIG. 3. SCHÖNBERG

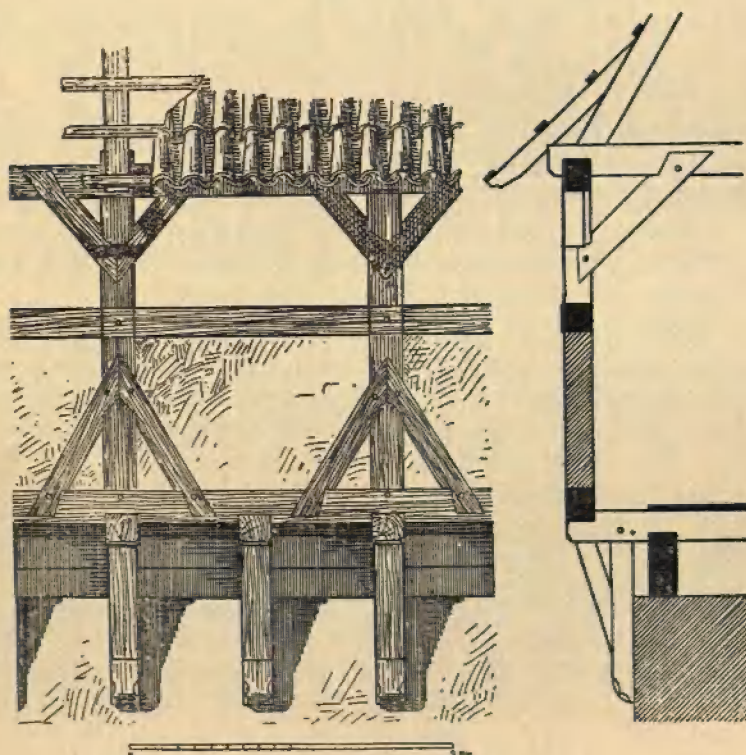


FIG. 4. SCHÖNBERG : detail of gallery

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Romanesque style. It is surrounded with a wooden gallery but the rest of the church shows no defence works. The east end is basilican in shape and dates from the original church.

TRAPPOLD (TRAPOLD). The church stands in a commanding position on the summit of a hill in the centre of the village. As can be seen from the plan (fig. 1), the walls are very strong and are further strengthened by several towers. This wall is double all round and the approach to the church resembles that at Birthälm, as shown on the plan, entering on the west side under a strong tower, then winding up the hill until it reaches the second gateway, where it bends left under a further tower of which the gateway is low. The space between the two walls is steep, but around the inside of the inner wall were store-rooms and living-rooms. The choir of the church itself is machicolated in the usual way with arches resting on buttresses having a stone loop-holed gallery above. The nave has loop-holes pierced close under the roof but is not machicolated. At the west end rises a massive tower, which has been built over the remains of the original church. It has a wooden gallery, which is in a good state of preservation. The church as it stands today was built in the second decade of the 16th century and further fortifications were added to the walls in 1638.

WALDHÜTTEN (VALCHID). Surrounded with a wall with three main towers. Over the entrance is a small tower with a pitch-spout, which bears in old German the appropriate text, 'Enter ye the narrow gate', etc. The tower in the west side of the wall has a wooden gallery and is known as the 'Bacon Tower', since the village bacon was stored here as at Neithausen. The church itself has no defences and the earthquake in 1916 brought down one of the towers in the wall. It has a good Gothic west door and the vaulting in the choir is in the same style.

WURMLOCH (VORUMLOCH). A very interesting and quite original church containing a combination of almost all the features found in these fortifications. Originally it was Romanesque in style but was altered to Gothic when it was fortified at the beginning of the 16th century, the fortifications being completed in 1526. A high wall with several towers surrounds the church, but it has now been considerably lowered and all the towers removed. The Romanesque west tower was defended by the addition of brick arches and a projecting gallery. In this respect it is unique, for in the case of the other churches, which have retained portions of the original structures, the new tower has

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been built complete around the old one, here on the other hand the tower has been left and only the buttresses added. The choir is built up to a great height—in fact almost as high as the west tower—and is machicolated by means of brick buttresses, which support the arches above; in addition it is provided with loop-holes for marksmen. The whole intervening nave is surrounded with the usual machicolation formed by arches resting on buttresses with a stone loop-holed gallery above. Tower-like erections rise above the north and south entrances to the church, which are machicolated and loop-holed. They could be closed by means of portcullises, that over the north entrance being still in place. The interior of the church has very fine Gothic vaulting and is one of the most beautiful I have seen in Transylvania. The original store-rooms and living-rooms still surround the wall on the inner side.

BIBLIOGRAPHICAL NOTE

The only articles in English which describe these churches are by Sir James Berry, F.S.A. He read a paper before the Society of Antiquaries of London on 3 April 1919 entitled 'Fortified churches of Southern Transylvania', which was published in the Society's *Proceedings*, series 2, XXXI, 165-76, with illustrations. Another paper, read before the Royal Geographical Society on 13 January 1919, on 'Ancient Dacia and Modern Roumania', is more general but refers incidentally to the fortified churches. It is printed in the *Geographical Journal*, LIII, 129-52.

The following works by German writers refer to the churches :—

Dr Fr. Muller, *Die Verteidigungskirchen in Siebenbürgen*. Wien, 1857.

G. D. Teutsch, *Geschichte der Siebenbürger Sachsen*. Hermannstadt, 1899.

O. Piper, *Burgenkunde*. München, 1905.

Emil Sigerus, *Siebenbürgisch-Sächsische Kirchenburgen*. Hermannstadt, 1923.

L. Eber, *Siebenbürgisch-Sächsische Kirchenburgen, Ungarische Rundschau*. Leipzig, 1912.

J. M. Salzer, *Birtheim in Siebenbürgen*. Wien, 1881.

On the map reproduced as fig. 1, the spellings conform with those of the R.G.S. Committee. On the other (fig. 2) they do not, but agree with the spellings in the text.—EDITOR.

Ancient Agriculture

by G. W. B. HUNTINGFORD

IN the first volume of *ANTIQUITY* appear two papers, one by Dr R. C. C. Clay, dealing with the formation of lynchets¹; the other, by Dr E. Cecil Curwen, containing a survey of prehistoric agriculture in Britain.² These papers, which are of considerable interest to the farmer as well as to the archaeologist, have suggested the following remarks, which I was unable to put on paper before as some of my books were in England.

It may be as well to note first of all the statements on which this paper will comment. Dr Curwen says that Early Iron Age and Romano-British ploughs had ploughshares consisting of a plain metal point fitted to the share-beam—such a plough merely scratches a groove (p. 268); lynchets were not formed intentionally (p. 273); a plough described by Pliny which turned a furrow did away with the need of cross-ploughing (p. 280); manuring is implied by rectangular fields with lynchets, and no visible evidence of two- or three-field rotation of crops (p. 286). In Dr Clay's paper, lynchets are defined as ploughed ground of which the natural slope has been altered by ploughing, which alteration was effected by ploughing the lower furrow only, bringing the plough back idle. This mode of ploughing flattened the slope, a slope being unsuitable for growing corn in a damp climate—the climate of Britain being damper in prehistoric times than it is now. (Pp. 57–59).

I. THE PLOUGH

The essential feature of the plough is that it turns a furrow by inverting the soil, whereas the cultivator or horse-hoe for working after the crop is sown merely stirs the soil without inverting it. Dr Curwen describes Early Iron Age and Romano-British ploughshares as consisting of a simple metal point³ which, he thinks, merely scratched a groove in the soil (pp. 268, 280). To this failure to turn over the soil he considers that we are to ascribe the need for cross-ploughing

¹ 'Some Prehistoric Ways', *ANTIQUITY*, 1927, I, 54–65.

² 'Prehistoric Agriculture in Britain', *ib.*, I, 261–89.

³ Our 'bar-point' share.

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advocated by Roman agricultural writers.⁴ This, as I shall endeavour to show, is not so. Further, I believe Dr Curwen to be mistaken in his idea of the ancient plough ; for, among other things, practical experience has shown me that no amount of cultivating, *i.e.* stirring the soil without inverting it, will produce a lynchet or terrace.

Till 1760, practically the whole of the plough was universally made of wood, and there is reason to believe that the shape of the plough changed but little during centuries of use.⁵ The most detailed account we possess of a Roman plough is Vergil's, which runs as follows :—

Continuo in silvis magna vi flexa domatur
In burim, et curvi formam accipit ulmus aratri.
Huic ab stirpe pedes temo protentus in octo,
Binae aures, duplici aptantur dentalia dorso.
Caeditur et tilia ante iugo levis, altaque fagus,
Stivaque, quae currus a tergo torqueat imos :
Et suspensa focis explorat robora fumus.

[In the first place, an elm is bent forcibly in the woods to the shape of a plough-body (*buris*), and takes the form of the crooked plough. From the base of this extends the beam (*temo*) for eight feet, and joined to it are two mould-boards (*aures*) and slades (*dentalia*) with double back. Beforehand, too, is cut the light lime for the yoke, and the tall beach for the stilt (*stiva*), which turns the bottom of the fore-carriage (*currus*)⁶ from behind : the wood is hung in the chimney to be seasoned by the smoke].⁷

To illustrate this plough, I append tracings from Martyn⁸ of two Italian ploughs in use in the 18th century, with another drawing of an old plough formerly used in Sussex. (Figs. 1, 2, 3).

Now, as regards the work done by the Roman plough, we have Vergil's express statement that

Pingue solum primis extemplo a mensibus anni
Fortes invertant tauri, glaebasque iacentis
Pulverulenta coquat maturis solibus aestas.⁹

⁴ e.g. Vergil ; Varro, R.R. I, XXIX, 2 ; and cf. Festus, s.v. Offringi, p. 523 (ed. Dacier, Valpy's ed., London, 1826).

⁵ Fream, *Elements of Agriculture*, ed. 10, p. 47. Fream is also mistaken, I think, in supposing that the old ploughs did not invert the soil.

⁶ I understand 'currus' as a wheeled forecarriage on the strength of Servius' comment. See Conington in loc ; see also Pliny, XVIII, §18, 48.

⁷ Verg., Georg., I, 169-175.

⁸ 'The Georgicks of Virgil', by John Martyn, F.R.S., ed. 5 (Oxford 1827), p. 40. Figs. 1, 2, 3.

⁹ Verg., Georg., I, 64.

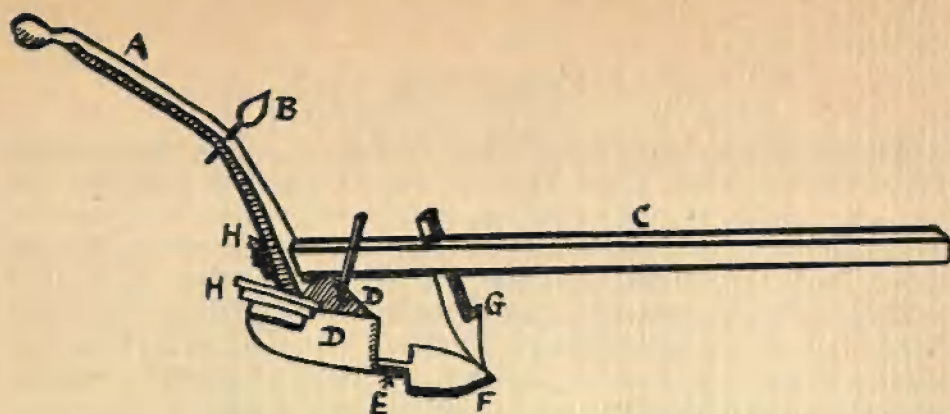


FIG. 1. EIGHTEENTH-CENTURY ITALIAN PLOUGH. (Martyn)

A, Buris. B, Stiva. C, Temo. D, Aures. E, Dentale. F, Vomer. G, Culter. H, Tabellae

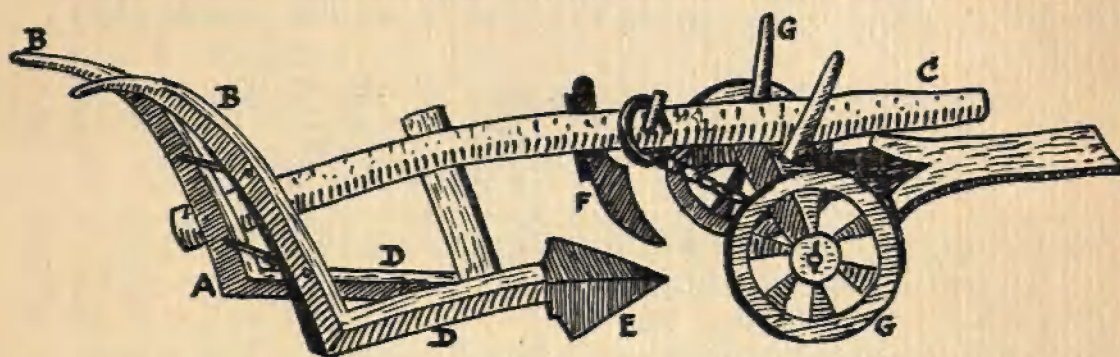


FIG. 2. EIGHTEENTH-CENTURY MANTUAN PLOUGH. (Martyn)

A, Buris. B, Stiva. C, Temo. D, Aures and dentale. E, Vomer. F, Culter. G, Currus

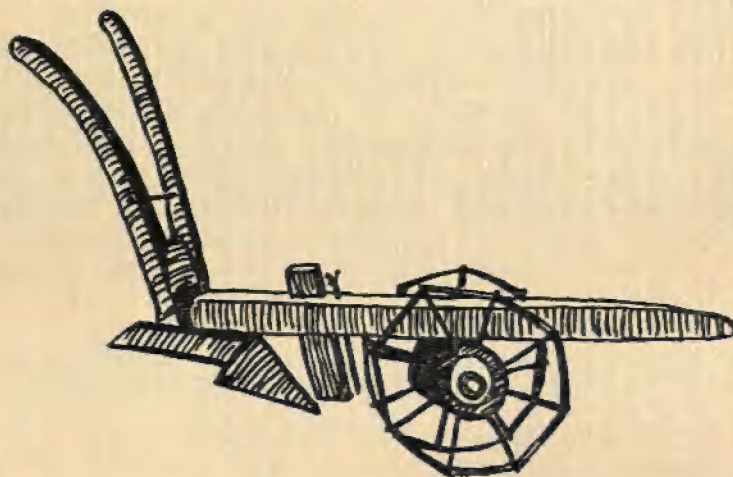


FIG. 3. OLD SUSSEX PLOUGH. (Reliquary)

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(Right from the beginning of the year the strong bullocks should turn over the rich soil, that dusty summer with its hot suns may bake the clods as they lie [upturned]). We learn from other sources something of the nature of ploughed furrows. 'Qua aratrum vomere lacunam striam fecit, sulcus vocatur. Quod est inter duos sulcos elata terra dicitur porca, quod ea seges frumentum porricit'.¹⁰ [Where the plough makes with its share a hollow furrow, the result is called sulcus (furrow). The raised earth between two furrows is called porca (ridge), because there the corn produces its crop]. And again, 'Porca est inter duos sulcos terra eminens',¹¹ (porca is the raised ground between two furrows); which is further defined by Columella, 'Liras rustici vocant easdem porcas cum sic aratum est ut inter duos latius distantes sulcos

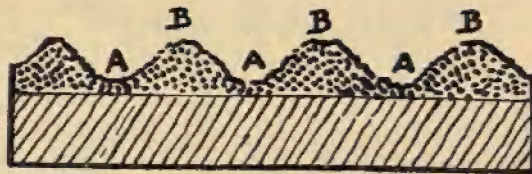


FIG. 4. DIAGRAM OF FURROWS.

AA, Sulcus ; BB, Porcae

medius cumulus siccam sedem frumentis praebeat',¹² (the country people call these same 'porcae' liras when the land is so ploughed that a heap of earth mid-way between two wide furrows offers a dry bed for the crop). This result (fig. 4), can only be obtained by using a plough which turns a furrow. Such a seed-bed was produced, so Varro tells us, 'cum tabellis additis ad vomerem',¹³ (with boards added to the share), *i.e.* not to the actual share, but to the mould-boards proper (fig. 1). The use of a coulter, too, even if not with every plough, implies the inversion of the soil, particularly as Pliny says 'culter vocatur, praedensam, priusquam proscindatur, terram secans, futurisque sulcis vestigia praescribens incisuris, quas resupinus'¹⁴ in

¹⁰ Varro, R.R., I, 29, §3. Ed. Goetz (Teubner).

¹¹ Festus, p. 319, s.v. Imporcitor. The porcae described above must not be confused with others 'aquae derivandae gratia' (ib. p. 611), which were open drains.

¹² R.R. II, 4, 8.

¹³ R.R. I, 29, §2.

¹⁴ Taking 'resupinus' proleptically.

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arando mordeat vomer',¹⁵ (that which cuts the heavy ground before the first ploughing is called a coulter, marking out a line for the future furrows by cutting into the ground, into which cutting the share penetrates turned back in ploughing). There is no other reasonable explanation of the term *porca* than that shown in fig. 4; and the statements of Varro, Vergil, Columella, Pliny, and Festus sufficiently indicate that the ground was actually turned over, not merely scratched. The only alternative is to suppose that every second furrow was left unploughed, which is absurd, and could not possibly have prompted Varro's etymology of *porca*, 'quod ea seges frumentum porricit',¹⁶ nor does this sanction White and Riddle's definition of *porca* as a 'balk'.

Dr Curwen considers (p. 280) that a plough which turns a furrow does away with the need for cross-ploughing. But, apart from the fact that the Roman plough did turn a furrow, we need not suppose that the terms *proscindere*=first ploughing, *offringere*=second or cross-ploughing ('to plough against'), and *lirare*=third ploughing (after or at the same time as sowing), need be taken at any period of Roman agriculture otherwise than literally: husbandry is the most conservative of all arts. And, though these methods are not now practised in England, the same procedure is adopted in maize-growing countries. Thus, in Kenya, our normal procedure is, where conditions permit, to plough and cross-plough, with possibly a third ploughing; the seed, too, is often planted behind the plough, and immediately harrowed in. And our ploughs are infinitely superior to anything the Romans devised. When the plough is drawn by oxen (whether the team be of two or fourteen) a cross-ploughing is often necessary because an ox-team does not plough as straight a furrow as a horse-team, and there are generally places which the plough has missed in the first ploughing. Further, the damper the climate, the worse the weeds, and one ploughing is often not enough to cover the weeds properly. Therefore, I maintain that the type of ox-drawn plough¹⁷ used—provided it turns a furrow—cannot be said to affect the need for cross-ploughing.

'Ploughshares of the Early Iron Age and Roman period are not

¹⁵ N.H. XVIII, §18, 48. And Pliny's words '*Latitudo vomeris cespites versat*' (loc. cit.) are conclusive.

¹⁶ The probable etymology of '*porca*' is *porcus*, and the literal meaning 'little pig'.

¹⁷ Only when a tractor is used is one ploughing sufficient under normal tropical conditions.

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uncommon, and consist of a simple metal point designed to fit on to the share-beam, without any device for undercutting and turning over the sods. Such a plough simply scratches a groove in the soil'. (Curwen, p. 268). But the discovery of metal shares only does not necessarily mean that the ploughs they belonged to did not possess other parts made of wood which could turn over the sods. The Roman plough was made of wood, and wooden mould-boards were used in Britain as late as 1830;¹⁸ and in the Sussex plough already referred to even the coulter is made of wood.¹⁹ Hence we may reasonably infer that people who had the intelligence to make an iron share, had also sufficient intelligence to provide their ploughs with some means of inverting the soil; for inversion is absolutely necessary in levelling a slope; and we may conclude that the mould-boards, together with the rest of the ploughs, have perished. A drawing in a MS of Caedmon's *Paraphrase* shows that the 'Vergilian' or Italian type of plough was used in England from early modern times; this plough has a mould-board. (Fig. 5).

When we approach the problem of ploughs of earlier periods than the Iron Age, we are on far less sure ground. That ploughs of a sort were used is quite clear; not so clear, whether they ploughed a furrow or scratched a groove.

The ploughs figured in the rock-carvings from the Maritime Alps²⁰ appear to possess a wooden bar which was driven through the ground (fig. 6), like the ancient Egyptian plough (fig. 7)²¹ which, we know, merely scratched a groove in the soil. On some of the megalithic remains near Carnac in Brittany are sculptured figures which M. le Rouzic believes to be ploughs ('hache-charrue'). Examples from the Dolmen des Marchands and the tumulus of Mané-er-H'roëk at Locmariaker are shown in figs. 8 and 9.²² These things, if they do represent ploughs, can have done no better work than the Alpine and Egyptian implements. My contention, however, that the ploughs which worked in the Early Iron Age fields had mould-boards, does not apply to ploughs of the Neolithic, Copper and Bronze Ages.

¹⁸ Fream, loc. cit., 47.

¹⁹ *Reliquary*, N.S. XI, 219.

²⁰ M. C. Burkitt, *Our Early Ancestors*, plate 28, fig. 1.

²¹ After Wilkinson, *Ancient Egyptians*, abridged ed., 1854, II, fig. 359.

²² Z. le Rouzic and C. Keller, *Locmariaker: La Table des Marchands*. (Nancy, 1910). My own copies of these are at present inaccessible to me; figs. 8 and 9 are therefore after Le Rouzic.

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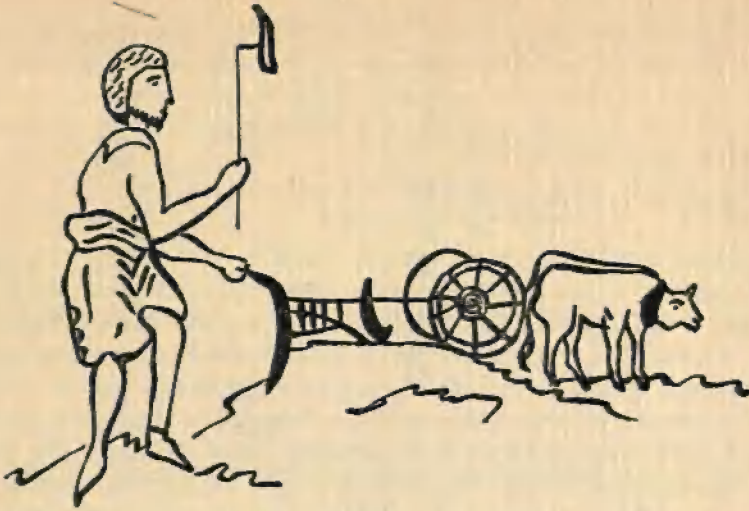


FIG. 5. 'VERGILIAN' TYPE OF PLOUGH. (Caedmon's 'Paraphrase')

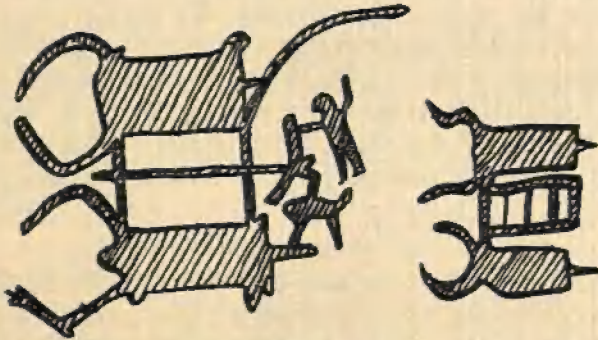


FIG. 6. PREHISTORIC PLOUGH AND HARROW. (Burkitt)
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FIG. 7. ANCIENT EGYPTIAN PLOUGH. (Wilkinson)

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2. THE HARROW

The plough implies the use of an implement to break the clods and render the land fit for seeding. The simplest and most primitive form of harrow was doubtless the 'bush-harrow'; though in the passage in Vergil,

Rastris glaebas qui frangit inertes,
Vimineasque trahit crates,²³

there is some doubt as to the interpretation of 'crates', which some consider means 'bush-harrow' (the 'arbutae crates' of l. 166), while others take it to mean 'osier hurdles'.²⁴ In any case, a bush-harrow is a most ineffectual implement, as I have proved from using one; and even primitive man would soon devise something better. Martyn's interpretation of 'crates' as 'hurdles' suggests that the rectangular implement with four cross-bars in Burkitt, plate 28, fig. 1 (fig. 6 *ante*)—which he does not mention in the text—may be a heavy hurdle used as a harrow.²⁵ On the other hand, it might conceivably (though perhaps with less probability) be regarded as a very early form of drag-harrow, with wooden teeth set in the beams, like the Roman *irpex*, 'Genus rastrorum ferreorum, quod plures habent dentes ad extirpandas herbas in agris'.²⁶ The shape is not mentioned; such harrows are generally triangular, and with iron or wooden teeth do quite good work. In the earliest representation known to me of this type of harrow (14th century), four transverse bars have teeth as well as the frame.²⁷

3. LYNCHETS OR TERRACES

The formation of lynchets, or as we now call them, terraces, is one of the objects of a coffee-planter in cultivating his plantation (Fig. 10). I grant that our terraces are only a fraction of the width of the Celtic lynchets, but the method employed, and the result, are the same; and as Dr Clay says (p. 57) they can be formed only by ploughing the lower furrow, so that all the soil is turned down-hill, bringing back the

²³ Georg. i, 95.

²⁴ So Martyn.

²⁵ Incidentally, one of the men in fig. 6 appears to be twisting an ox's tail, a common method in Africa of inducing a lazy ox to move.

²⁶ Festus, p. 339, s.v. *Irpices*. 'A kind of iron rake with many teeth for tearing out weeds in the fields'.

²⁷ In a 'Shepherd's Calendar', in the Brit. Mus.; figured in Ditchfield, *Old Village Life*, p. 137.

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FIG. 8. CARVING OF PLOUGH, 'DOLMEN DES MARCHANDS'. (Le Rouzic)

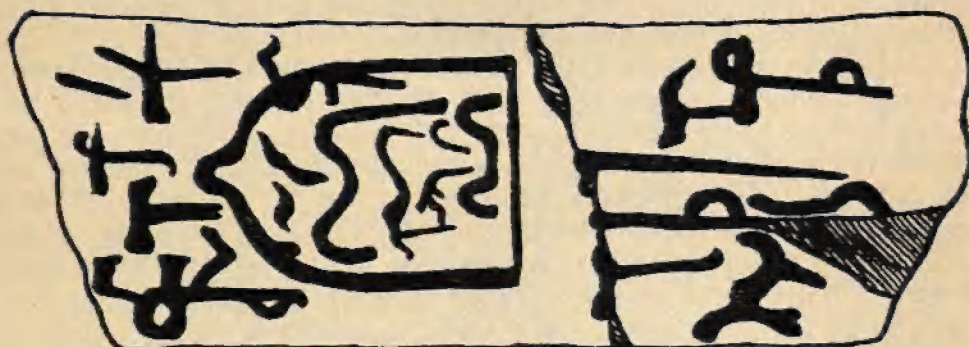


FIG. 9. CARVINGS OF PLOUGHS, MANÉ-ER-H'ROËK. (Le Rouzic)



FIG. 10. TERRACES, KENYA COFFEE PLANTATION
A-B, natural slope

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plough idle instead of ploughing the upper furrow. Nowadays we use a 'one-way' or 'hillside' plough, which has a reversible share held in place by a catch; at the end of each furrow the share is swung under the plough into position for the next furrow.

Our 'lynchets', on a slope such as shown in fig. 10, have a negative lynchet 2 ft. high; this can be produced in about two years by 3 to 4 ploughings a year, the level strip being disc-harrowed after each ploughing. On a rough estimate, one might put the formation of a Celtic lynchet 10 ft. high and 200 ft. wide at about 40 years, if it received two ploughings a year; if it was only ploughed once a year the time would be longer. Once a coffee terrace has been properly formed, it does not need to be ploughed with a 'one-way' plough every time, as an ordinary plough will preserve the proper level. So prehistoric man probably did not find it necessary to plough the lower furrow only every time, once his lynchet was established.

Our object in levelling the slope by terracing is to stop soil-wash, the worst thing in nature with which the planter has to contend; and I can see no other reason for the ancient lynchets. The mere fact that ground is sloping does not make it unsuitable for growing corn in wet climates, as Dr Clay says (p. 58). For in East Africa, native fields of maize, millet, and the short-stalked *eleusine coracana*, are often found on very steep slopes; and as long as they are prepared by hand (with 'hoes') the soil washes very little in comparison with ploughed land. It is when the plough comes into use that soil-wash starts; hence the necessity of flattening the slope. The formation of lynchets therefore implies that soil-wash occurred, and that they were deliberately formed to prevent it.²⁸

4. MANURING AND ROTATION

It is not quite clear what Dr Curwen means by his statement that there is 'no visible evidence of two-field or three-field rotation of crops' (p. 286), for a number of fields side by side need not imply any sort of rotation. Even if the Britons marled their land with chalk,²⁹ and the Welsh spread wood-ash on their fields,³⁰ there are no grounds for supposing that they had any sort of rotation; for 'the Romans

²⁸ Dr Curwen considers that lynchets were not formed intentionally (p. 273).

²⁹ Pliny, N.H. XVII, §6, 4, seq.

³⁰ *Mabinogion*; quoted by Curwen, p. 287.

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seem to have had some glimpses of rotation of crops ; but it does not appear that any system of agriculture founded upon this knowledge was in general use among them'.³¹

Perhaps something can be learnt in this connexion from native agricultural methods in Africa ; and the following account of Nandi³² agriculture will perhaps suggest methods which may have been practised by the more primitive of the early inhabitants of Britain. The Nandi when preparing new ground for eleusine corn, break up the turf with hoes. The turf is then collected into small heaps, and fired, and left to smoulder till the grass is burnt out.³³ The heaps of burnt earth are then scattered over the ground, dug in with hoes, and a tith is prepared for the seed. The usual method of planting is to have a little of everything in the same patch—eleusine, millet, maize, beans, and sweet potatoes. Next year the stubble is dug over, and maize and millet planted, but not eleusine ; for the Nandi consider that the ground must be pared and burnt every year for this crop. So fresh ground is prepared. After four or five years the patch is abandoned, and new ground taken up. The cultivated fields are communal, and each man's holding is a strip separated from the next holding by a narrow path.

We have here no rotation, but some idea of the manurial value of wood-ash. The idea of leaving a patch of cultivated land after a few years is due, not so much to a recognition of the benefit derived from fallowing, as to the repressed nomadic instincts of the Nandi, for they frequently move house as well. The natives in this part of the world have not attained to the plough because a small area only need be cultivated to support a family, and each man grows his own food. The plough is the production of harder climates where a larger area is needed under primitive methods of tillage than can conveniently be prepared by hand.

³¹ Daubeny, *Lectures on Roman Husbandry*, p. 124, quoted by Conington, *Vergil* (Bibl. Class.), 1, 159.

³² I take the Nandi as an example although they are a pastoral tribe, because their agricultural system (borrowed from the Bantu Kavirondo) is very well defined ; and because I am much better acquainted with their methods than with those of other tribes.

³³ The old-fashioned process of paring and burning formerly practised in England.

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THE ARCHAEOLOGIST AND THE MAN IN THE STREET

The following newspaper quotation¹ is an opinion about archaeology so common as to constitute a challenge worth accepting:—

‘ HOW SHOULD THEY DO IT ? ’

‘ There are other questions easier than that of the Vallum.

‘ Why was a naval detachment at Birdoswald ? Did Tacitus ever visit Britain ? When and where was the Ninth Legion annihilated ? How were messages sent from Britain to Rome in a few hours ? How did the Romans, with the primitive tools at their disposal, manage to quarry and shape the enormous quantities of stone required and perform the monumental labour of building the Wall in two or three years ? A tee-piece of the finest tool steel was found in Vinovia ; whence came it, or how was it made and by whom ?

‘ All those are fair questions. So long as the experts are compelled to submit theories as answers, they cannot afford to be contemptuous of inexpert theories, which, at least, frequently embody the virtues of interest, intelligent imagination and a respect for the probable ’.

How does the notion arise that, short of a solution, one opinion about a riddle is as good as another ? It is belied by human experience as a whole. No deep thinker admits its validity, but if his subject looks easy, it is often expected that he should. In fact, this is an old question. Is authority always right, and is ignorance always wrong ? On the side of ignorance, let us admit that ‘ fools step in where angels fear to tread ’, and sometimes succeed ; on the side of authority, that familiarity with any problem, from coal-hewing to philosophy, entitles the expert therein to respect. But each side has its special temptation. Ignorance presumes upon success, and authority tends to pontificate. The resultant confusion gives rise to the notion which we questioned.

But does the archaeologist spend his time theorizing about unanswerable riddles ? If he does, he is not doing his proper work, which is to collect, by observation or excavation, the facts which bear upon them. For example, asked why a naval detachment was at Birdoswald, he would reply that at least two inscriptions² record marines helping to build Hadrian’s Wall, a work in which varied drafts³

¹ *North Mail and Newcastle Chronicle*, 26 April 1932.

² *Corpus Inscriptionum Latinarum*, VII, 864, and a new stone discovered by Mr E. B. Birley this spring.

³ cf. *C.I.L.*, VII, 775 (Dumnonians), 863 (Catuvellaunians).

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were engaged, thus permitting the facts to tell their own tale. His only merit lies in having come by the facts.

The question whether Tacitus ever visited Britain would receive the answer that little is known of Tacitus outside his books, and nothing which suggests that Tacitus was ever in the island. If anyone said that Tacitus could have paid a visit to his father-in-law Agricola, the Governor of Britain, no one would deny that this idea possessed both 'interest, intelligent imagination and a respect for the probable'. But the expert would abstain from the theory.

Nor would he waste time asking when and where the Ninth Legion was annihilated. An inscription⁴ of A.D. 108-9 is the last record of this Legion in its headquarters at York. After that date it appears nowhere else, and its place is taken, not later than A.D. 125, by the Sixth.⁵ Again, Fronto⁶ mentions heavy casualties in the British Army in Hadrian's first years. All this roughly dates the disappearance of the Legion. But who can say whether it fell in battle or was cashiered, wholly or in part? And if its end is doubtful the question where it happened is premature. Still less is any theory required about messages being transmitted from Britain to Rome in a few hours, since no ancient source supports the notion.

Roman quarry methods are no mystery. They can be studied in many places⁷ throughout the Empire, including Hadrian's Wall. Actual Roman tools⁸ exist, in wrought and tempered iron, and Roman sculptured representations⁹ show many types. These tools are like the modern ones, primitive because they are simple. Roman cranes and derricks for handling stone are described in literature¹⁰ and appear in sculpture.¹¹

⁴ C.I.L., VII, 241.

⁵ Parker, *The Roman Legions*, pp. 161-2; Ritterling, *Paulys Realencyclopädie*, XII, col. 106.

⁶ Fronto, ed. Naber, p. 218; *Hadriano imperium obtinente quantum militum a Britannis caesum*.

⁷ Odenwald, Germany: Neuburger, *The technical arts and sciences of the ancients*, pp. 400-1, figs. 542-7; El Medol, Spain: Mérida, *Monumentos romanos de España*, pl. 1; Rome: *Bolletino Comunale*, 1888, tav. I-II. No proper account exists of the quarries on Hadrian's Wall at Shawk, Coombe Crag, Haltwhistle Fell, Limestone Bank and Fallowfield Fell.

⁸ Collingwood, *Archaeology of Roman Britain*, fig. 65t, chisel; Curle, *A Roman frontier post and its people*, pl. LVII, axes.

⁹ Neuburger, *op. cit.*, p. 396, fig. 538.

¹⁰ Vitruvius, *De Architectura*, x, 2; in Morgan's translation, pp. 285-90. For stone saw-mills, worked by water-power, see Ausonius, *Mosella* 363, *stridentesque trahens per levia marmora serras*. (Note 11 on p. 340)

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The 'tee-piece of finest tool steel' from Binchester is not surprising. These iron cramps¹² were not uncommon, and habitually of good metal; but analysis¹³ would probably reveal less obvious differences between it and tool steel.

The Vallum, which produced this kettle of fish, has been the object of much recent field-work and excavation.¹⁴ Many theories have been made about it, but not by experts. On the Vallum only one expert exists, namely, Mr F. G. Simpson, who has thoroughly studied its characteristics: and, after stating the problem,¹⁵ he has been quietly gathering more facts about it.

The new facts, observed data whose meaning is clear, add much to knowledge, but are too few to provide a comprehensive account. Less thought goes to securing them than to defining the given problem and selecting the spot for its solution.¹⁶ Therein lies the need for detailed knowledge of the monument, upon which the expert bases his claim to recognition.

Finally, no expert despises those who would study the facts with him, but the necessity for such study is preparedness in the terms. For such research leaves little time for imparting its methods, and none for irrelevant argument. Indeed, the methods themselves are often experimental, as in any similar science. This state of affairs forms the real argument in favour of the recent proposal to found an Institution for teaching archaeological method. When more people know it, more will expound it, and the public will not then fail to appreciate either the scientific value of archaeology or the exacting training that it requires. Meanwhile, it will keep its place in the public estimation as a pursuit exciting rather than exacting, spectacular rather than sober, and simple rather than highly complicated. I. A. RICHMOND.

¹¹ Crane: Relief of Haterii, Strong, *Art in Ancient Rome*, II, p. 65, fig. 343 and *Legacy of Rome*, p. 305, fig. 23. Derrick: Lugli, *Forma Italiae*, I, pl. I and Reinach, *Repertoire de reliefs grecs et romains*, p. 422, I.

¹² Durm, *Baukunst der Römer*, p. 206, figs. 209, 210: Vitruvius, *De Architectura*, II, 8, 4.

¹³ See discussion in Blümner, *Technologie*, IV, 349-52.

¹⁴ Simpson and Richmond, *Excavations on Hadrian's Wall in the Gilsland-Birdoswald-Pike Hill sector*, 1928, *Cumberland & Westmorland Antiq. & Arch. Soc. Trans.*, N.S. XXIX, 303-15; Simpson and Bosanquet, *Northumberland County History*, XIII, 484-565.

¹⁵ Simpson and Shaw, *The purpose and date of the Vallum and its crossings*, *C. & W. A. & A. Soc. Trans.*, N.S., XXII, 353-433.

¹⁶ For an amplification of this view, see Collingwood, *Journal of Roman Studies*, XXI, 36-8.

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THE LION AND THE UNICORN

In his very interesting article entitled 'The Lion and the Unicorn', printed in *ANTIQUITY*, December 1930, Mr CYRIL G. E. BUNT* gives an astronomical and religious origin to this ancient legend.

He points out that, 'to the Sumerians, Babylonians and Assyrians—indeed to all primitive people—one of the chief events ever recurrent in their lives was the coming of Spring. The sequence of the seasons is personified in their hierarchy, and, as a preliminary to the full effulgence of summer, the Sun-God finally vanquishes the powers of Winter during the Season of Spring. . . The most significant fact, for us, is found to be, that, at this period, when the Summer solstice was in Leo (the Lion) the spring equinox was in Taurus (the Bull). Spring, the bringer of new life to the earth, was inevitably followed by Summer's fierceness. Thus we may say that the lion and bull (generally with one horn, be it remembered) symbolize the triumph of Summer over Spring'. I would suggest that this astronomical symbolism of the myth is a later, probably Babylonian, development, and that an earlier form of the myth is to be found in 'The Epic of Gilgamesh' and is depicted on the earliest cylinder-seals of Sumer and Akkad, which date back to about 3000 B.C.

In the 'Epic', Gilgamesh, who was a legendary king of Erech and of divine origin, incurred the wrath of the goddess, whom the Babylonians called Ishtar. In revenge for the slight which Gilgamesh inflicted upon the goddess, she caused a bull of unsurpassed strength to be created, and sent it to attack and kill the hero. Gilgamesh, with the assistance of his friend Enkidu, overcame and slew the bull, but Ishtar gave the credit of the slaying to Gilgamesh, who by this act, established his position with the people as hero and king. Enkidu was the strong man, who lived with wild beasts and finally succumbed to the charms of the courtesan who was sent to tame him. After Enkidu surrendered to the courtesan, the wild beasts fled from him and he came and dwelt with the shepherds, and, being of great strength, he hunted and slew the lions which harried the flocks of the shepherds. On the cylinder-seals of Sumer and Akkad, which date back to 3000 B.C., we find, depicted over and over again, the scene of Gilgamesh, the king, in conflict with the bull and Enkidu in conflict with the lion. The next step in pictorial representation appears to be the conflict between the lion and the bull (often represented with one horn). Mr Bunt

* We should be glad if Mr Bunt will send us his address.—EDITOR.

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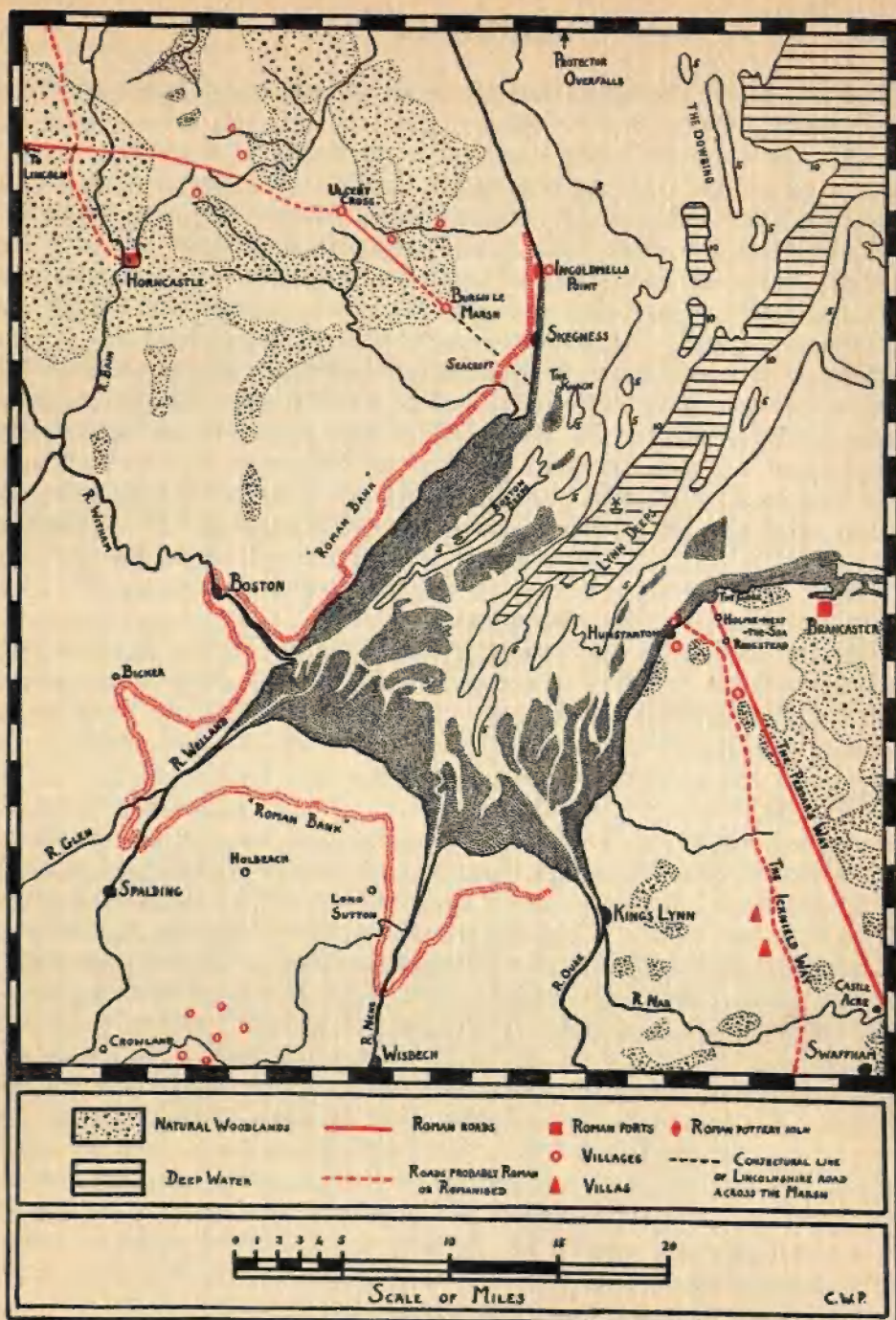
has given many illustrations of this conflict, which is the commonest motive on the seals of all the Mesopotamian cultures. He has pointed out the equation between the sun-god Shamash, and the lion, which was his symbol. But the lion was not only the symbol of the god, but also of the king. The earliest kings of Sumer, such as Ningirsu of Lagash, who was also a god, the eldest born of Enlil, had a lion-headed bird as his emblem. The lion-head also appears on the mace-heads of Mesilim and Gudea, both kings of Sumerian city-states and of divine parentage. Therefore, it is reasonable to equate the lion with the king as well as with the god, since the kings of Sumer and Babylon were essentially king-gods. Hence it is possible that the representations of the lion fighting the unicorn (bull), symbolized the account in the 'Epic of Gilgamesh' of the king slaying the bull sent by Ishtar to destroy him. If this is so, the astronomical symbolism was a later Babylonian development which was added to the original and simpler legend. Any explanation of the legend of the fight between the lion and the unicorn must take into account the association of the king-god and the lion from the earliest period, in conjunction with the account in 'The Epic of Gilgamesh', depicted on the cylinder-seals, of Gilgamesh who overcame and slew the bull, and of Enkidu, his friend, who was a slayer of lions.

Mrs C. N. DEEDES.

THE ROMAN FERRY ACROSS THE WASH

The publication in 1928 of the second edition of the Ordnance Survey map of Roman Britain was followed by its review in ANTIQUITY by Dr R. E. M. Wheeler who, among other things, showed how the map made intelligible the apparently aimless behaviour of the Peddar's Way in running to the coast on the northwest corner of Norfolk, where there is no known Roman settlement of any importance save the Saxon Shore fort of Brancaster which lies 5 miles east of the point where the road meets the sea. The explanation is found in the existence of a Roman road running east and southeast from Lincoln to meet the opposite shore of the Wash in the neighbourhood of Skegness, which would seem to postulate some sort of a ferry across the Wash in Roman times. The credit for having first pointed out this possibility belongs to Canon E. H. R. Tatham, formerly vicar of Well. (*Memorials of Old Lincolnshire*, 1911, p. 49).

A glance at the map shows that the force of the ferry-theory is inescapable, but there are certain problems which will have to be



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solved before the matter is in a satisfactory state, and it is the object of this note to point out their nature.

The first question to be settled is where the two roads meet the present coast line. In the case of the Peddar's Way this would seem to be simple since it appears to go without a deviation right to the beach at Holme-next-the-Sea, but it is by no means certain whether the existing stretch from Ringstead to Holme represents the original line. During the dry part of the year 1929 Mr O. G. S. Crawford and Wing-Commander Insall, v.c., made an observation on the ground that on the north side of the village of Ringstead, just before the line of the Peddar's Way is taken up once more by a modern road, a broad band of parched grass veered away to the westward in the direction of Hunstanton as if to join the modern road which runs that way, and to terminate somewhere on the cliffs to the north of the modern resort. At Holme-next-the-Sea there is some hummocky ground in the line of the Peddar's Way on the seaward side of the village which is called 'British Encampment' on the 6-inch Ordnance Sheet, but this identification is entirely without authority.

If the Peddar's Way really passed through Holme it must have encountered the modern sea coast at the inlet known as the Gore. One thing at least is certain—that the Way reached the modern sea coast somewhere between Hunstanton and Holme and therefore at some point along a front of only two miles.

The problem of the coast termination of the Lincolnshire road is much more difficult. The line of the road from Lincoln may be traced with certainty past Wragby, Hatton, and Sturton, leaving Horncastle to the south and mounting on to the southern end of the Wolds across Flint Hill to Belchford and Tetford. From here its course to a considerable Romano-British settlement at Ulceby Cross above Alford has not been finally determined, but from Ulceby Cross it takes a decided turn to the southeast and is last clearly visible by Welton vicarage, pointing in the direction of the townlet of Burgh-le-Marsh which stands on a hillock dominating the coastal plain known as the Marsh. There can be little doubt that the road reached Burgh, but from here to the coast, a matter of five miles, its course is quite unknown. The name of Burgh seems to promise a Roman settlement, but there is practically no evidence that there was ever more than a slight occupation of the site in Roman times. The Roman material found on the site seems to be confined to coins of the Antonines which have been discovered on the site of the churchyard of the ruined church of St. Mary.

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The only Roman site of any importance or authenticity along the coast here is at Ingoldmells Point, where considerable quantities of Roman rubbish were regularly exposed by the erosion of the sea until the erection of great coast protection works at this point sealed down the site with concrete. The pottery from this site has been identified by Dr Felix Oswald as dating from the first to the third centuries. The site is too far north of Burgh to be likely as a terminal point of the road, especially since it has recently been proved that it was probably not strictly on the open sea at all in Roman times.

If the road was making for the end of a ferry it is likely to have continued in the same line as from Welton to Burgh, and so reached the modern coast line at Seacroft south of Skegness and opposite the considerable sand banks known as the Inner and Outer Knock, which are probably an ancient extension of Gibraltar Point now overwhelmed by the sea. There is no recognizable trace of the road across the Marsh now, but it may not be without significance that the line of the modern road following the Cow Bank drain towards the sea past Seacroft station is continued by the parish boundary of Skegness out on to the beach in a straight line and points to the northern end of the Knock Bank.

Traditions and documentary evidence both point to the disappearance under the sea of some place of importance, possibly Roman. When Leland visited the coast during his tour he was told that at Skegness there had once been 'a great Haven towne' with a castle and surrounding wall, but it was 'clene consumed and eten up with the se'. Tradition names this place 'Wilegripe' and places it at a point four miles out into the Wash. More definite evidence of the disappearance of a Roman site is provided by the mention in the Ingoldmells manorial rolls of a piece of land called 'Chesterland' or 'Castelland' which is mentioned by both names indifferently in 1345, and again in 1422 four acres of land in 'Chesterland' are mentioned as being surrendered by one William Skalflete (*Court Rolls*, p. 248). This is the last mention of this land, and it is to be presumed that it was swallowed up by the sea in the great inroads of the later Middle Ages.

Roman pottery may be found here and there along the coast from Chapel St. Leonard's southwards to Gibraltar Point, and it is probable that the greater part of it has washed out of the Ingoldmells site, but it is impossible to exclude the chance that it may come from drowned sites. A Skegness fisherman, now dead, claimed to have brought up

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Roman pottery in nets from off the Knock, but, whatever he found, it does not survive.

In spite of the identification of Wainfleet with the mythical VAINONA by the Ordnance Survey there is not a particle of evidence for any Roman site there. The so-called Roman salt works south of the town is a site which is well worth proper investigation with the spade but, although there is abundant evidence in the long banks of calcined material that some fairly extensive enterprise has been conducted here, a diligent search of the surface by the writer has not produced a single datable object, much less the quantities of Roman pot rubbish which would almost certainly be found there if the works have a Roman origin. This site seems to have affinities with the Red Hills of Essex.

Thus up to the summer of 1931 the surface evidence of this part of the Lincolnshire coast has not justified the identification of any existing site either with the Roman road or its destination, the presumption being that both have been lost under the sea.

Professor H. H. Swinnerton has recorded¹ the results of work on the recent geology of the Lincolnshire coast and made reference to the traces of human settlement both before and during the Roman occupation. It is not our purpose to concern ourselves in detail with his conclusions about the state of the coast in the Bronze and Iron Ages, but he makes it abundantly clear that in Roman times the coast was comparatively sheltered from the full force of the North Sea in strong contrast to its present exposed condition. When the trees of the Forest Bed which underlies the peat exposed along this coast were growing he considers that the land had an elevation of 25 feet above its present level. At the close of the Bronze Age there was a slight elevation, but 'on the whole it would seem that little or no downward movement took place between the close of the Bronze Age and the later years of the Roman occupation'. He goes on to say. 'The existence of *Scrobicularia* and *Cardium* at intervals along the whole length of this strip of coast together with the subsequent accumulation of the upper post-glacial clays shows that for a long time after the departure of the Romans this strip must have occupied a sheltered estuarine situation. Today it lies exposed to the full onslaughts of the North Sea waves'.

¹ 'The post-glacial deposits of the Lincolnshire Coast', by Dr H. H. Swinnerton. Quarterly Journal of the Geological Society, vol. LXXXVII, part 2. 1931.

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The existence of numerous sites on the foreshore along this part of the coast where sea water appears to have been boiled down for salt in the late Bronze Age and the Iron Age make it unlikely that the coast line varied very greatly from its present trend. No one would take the trouble to carry the water a long way before treating it and so it may safely be assumed that it was not far from these sites. The Roman site at Ingoldmells Point takes advantage of a large hummock of boulder clay which gives a slight elevation over the surrounding Marsh and has resisted the erosion of the sea more effectively in modern times. Its occupants looked out on the sea at no great distance but it was not the North Sea in all its strength which broke on their coast. The shoals called the Protector Overfalls, Docking, and Dowsing were then an effective barrier to the irruption of the full force of the sea, with the result that this coast was fringed by what amounted to a great brackish lagoon into which flowed the main ocean at high tides. Under conditions of this kind shell-fish like the cockle (*Cardium edule*) could flourish and clays be deposited which now would have no chance to form. It is Professor Swinnerton's opinion that the change in the circumstances of this coast is not due so much to any subsequent sinking of the North Sea bed which carried the shoals below the height of effective protection, as to their progressive erosion by the sea, which reached a stage in the late 13th and early 14th centuries at which there was no effective resistance to an unusually high tide with the result that disastrous floods afflicted the Lincolnshire Marsh. These floods are a matter of history.

The strong probability that this was the course of events makes it necessary to review the position of the so-called ' Roman Bank ' which runs round the coast from north of Ingoldmells Point to the neighbourhood of King's Lynn. The need for this coast protection can only have come into existence with the change of conditions on the coast and it has always been the opinion of the writer that the work is medieval and has been constructed section by section locally. It is a suspicious circumstance that the work shows very little regularity and plan and that along its whole extensive line there are no records of Roman finds having been made in association with it. Knowing the amount of rubbish which cumbers even meagre Romano-British sites, it is remarkable that, granted the Roman authorship of the Bank, there are no points along its line where there are traces of the large concentrations of men who must have been employed to throw it up. It is also an awkward fact that the two Roman sites which either were or may be

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assumed to have been on this coast—Ingoldmells Point and the lost site near Skegness—are both on the seaward side of this protective line.

Air-photographs taken over the Cambridgeshire and Lincolnshire Fens recently have shown that, far from being an uninhabited waste in Roman times, there were areas occupied by native villages almost as thickly scattered as those on Salisbury Plain at the same period. This applies especially to the silt-ridge south of Whaplode and Gedney east of Crowland as well as to the line of the old Great Ouse between Littleport and Wisbech. It is hoped that air-photograph mosaics of larger areas will yield many more sites.

We now return to the question of the possibility of a Roman ferry across the Wash. At the present time there is no more difficult crossing for sailing vessels on the East Coast than that of the mouth of the Wash from Norfolk to Lincolnshire. This statement is made on the authority of highly experienced yachtsmen who know the waters well, and a ferry would be quite impracticable at the present day because of the strong tides, dangerous sandbanks, and numerous currents. If a crossing was regularly made here in Roman times as part of a system of communication the conditions must have been much more favourable than now. The present width of the Wash across its mouth is now 14 miles and if the Knock is regarded as land which was probably above the surface in Roman times the width is reduced to 10 miles. The protection afforded to the Lincolnshire coast by shoals which were in all probability matched on the Norfolk side by the huge overhang of shallow water, quite 18 miles long, stretching north from Brancaster was considerable, and in view of this it is impossible to be sure just how wide the effective mouth of the Wash was in Roman times. It is a fairly safe speculation that it was considerably less wide than it is now so that the crossing was shorter, and the strong current from the north down the Lincolnshire coast can hardly have existed with the Protector Overfalls higher than they are now. The matter may be summarized by saying that on the Lincolnshire coast, at least, the terminal point was probably well below modern low water mark and that the same may have been the case in a lesser degree on the Norfolk side. A crossing shorter in distance than the modern one was thus made across waters which were strongly tidal but which were comparatively land-locked. It is unlikely that the sunk sites will ever be identified but a piece of work which can and should be done is the working out of the age of the roads which approach the ends of the crossing and that of the settlements along them. C. W. PHILLIPS.



WANSDYKE
Pitt-Rivers' section can be seen near the middle
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GENERAL PITT-RIVERS' SECTION OF WANSDYKE

In 1889 General Pitt-Rivers dug a section across Wansdyke at a point three-quarters of a mile NW of Shepherd's Shore (where Wansdyke is crossed by the Devizes and Marlborough road). This point lies just over a quarter of a mile NW of Old Shepherd's Shore. In the outer bank on the counter-scarp the section revealed what was plainly the ditch and bank of an older entrenchment. For once the General seems to have failed to have interpreted this correctly. He says :— ' The eastern side of the cutting appears to have cut through a large heap of chalk rubble, B, which was thrown up during the formation of the rampart. The relics above and below this line of dark mould were of precisely the same character, and the dark seam here appears to have been caused either by heaping up turf cut from the ditch, or by a deposit of surface mould thrown up undesignedly during the original construction of the rampart '.*

At this point, however, for a distance of about a quarter of a mile, Wansdyke coincides with an earlier entrenchment which was marked on the original one-inch Ordnance map (about 1806) but not on the subsequent large scale maps, until I had it re-inserted on the edition of 1925-6. The entrenchment in question runs roughly NW-SE ; starting from the Roman road at a point immediately NE of Horsecombe, it traverses Morgan's Hill, where it consists of two ditches and three banks. It is evident that the builders of Wansdyke utilized a portion of this linear earthwork for about a quarter of a mile. The mutual relationship between the dyke and the linear earthwork is well brought out by the accompanying illustration. On the south side the earthwork has been mutilated by some flint diggings, but it can still be traced both on the ground and from the air as far as the old Bath road, 750 ft. SW of Old Shepherd's Shore.

It will be plain that the relics found below the dark seam BR are not only older than the construction of Wansdyke, but also than that of the older earthwork. Since, however, a fragment of Samian ware was found in the chalk rubble forming one of the banks of the older earthwork, both earthworks would appear to have been constructed in or after the Roman period.

The General states specifically in the passage quoted above that there was no difference in character between the finds above and below the dark mould.

*Pitt-Rivers, *Excavations in Cranborne Chase*, 1892, III, 252.

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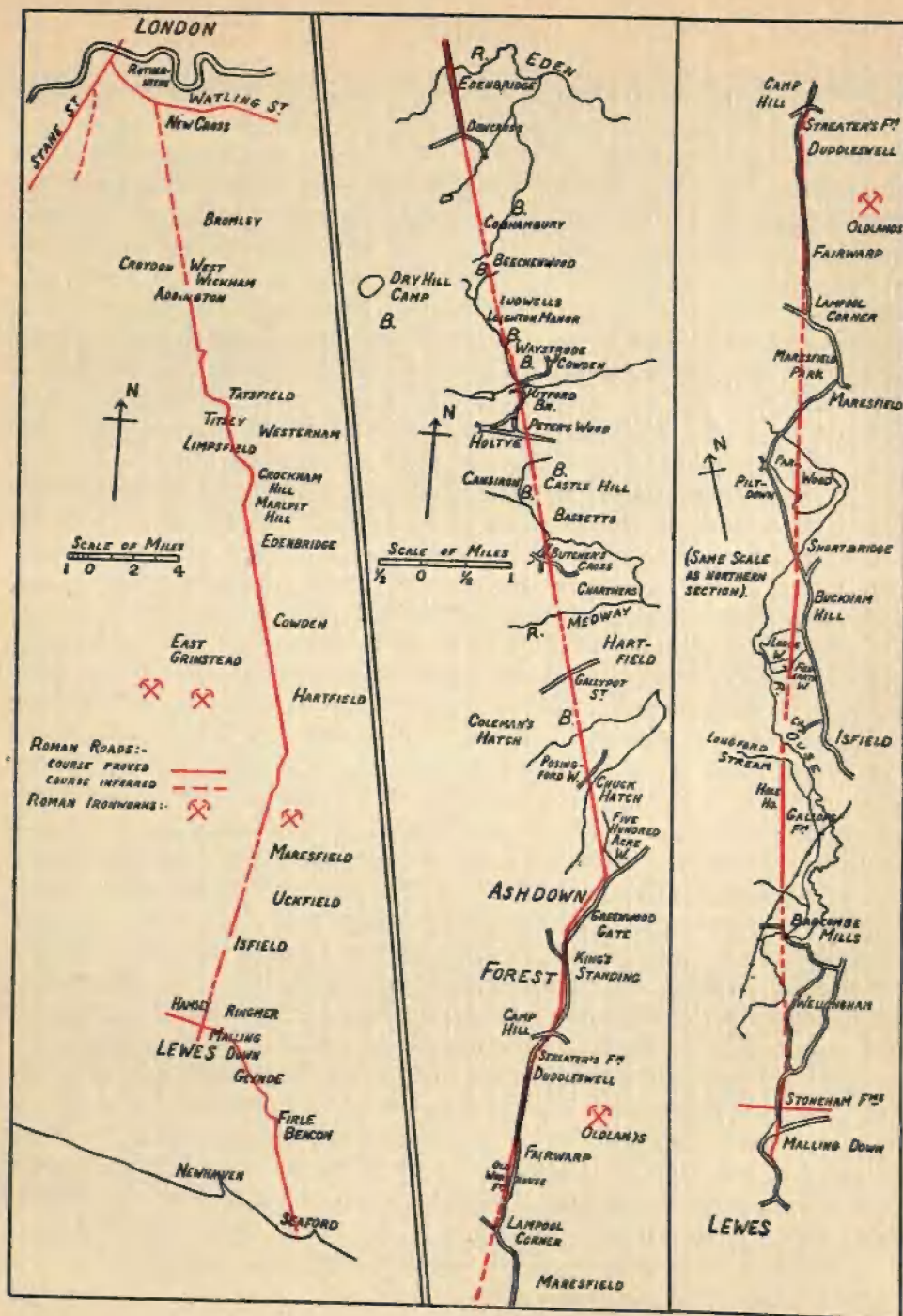
The remains of what might have been thought to be an older ditch were noticed in the main rampart itself, but the General denied this. He may well have been correct in this, for it would appear that the rampart lay too far to the south for any portion of the older entrenchment to have been included beneath it. In any case it is quite certain that the fragment on the counter-scarp itself represents the remains of this tri-vallate earthwork; and it might be worth while examining objects discovered in association with it again in the light of the knowledge of Romano-British and Iron Age relics which has accumulated during the past 30 or 40 years. O.G.S.C.

ROMAN ROAD FROM LONDON TO SUSSEX

Recent discoveries, initiated by air-photography on Ashdown forest, have enabled the entire course of a new Roman road to be traced through Surrey, Kent and Sussex from West Wickham, near Croydon, through Edenbridge to Maresfield and thence down the valley of the Ouse to Malling Down, east of Lewes, where it connects with roads in the South Down area. Two detached portions of this route had previously been identified as Roman; the northern forming the Kent-Surrey boundary for $4\frac{3}{4}$ miles in a straight line from Wickham Court near Addington to Coldharbour Green on the escarpment of the North Downs above Titsey, and the southern (lying parallel with the other but half a mile farther east) from Marlpit Hill across the Eden valley at Edenbridge and up the slope of the ridge beyond, a distance of some 3 miles.

Investigations by Mr James Graham have established the portion connecting these two sections, but it is with that part of the route lying between Edenbridge and Lewes that the writer is directly concerned. A large hill-top camp, probably of Early Iron Age, with triple ramparts, crowns Dry Hill on the ridge to the south of the Eden valley at a point about $1\frac{1}{4}$ miles west of the Roman road, but until quite recently no other ancient sites had been discovered anywhere near the course of the road until the well-known Roman ironworks site at Oldlands near Maresfield is reached. The unpromising nature of the country had presumably checked investigation and no efforts appear to have been made to discover a southward continuation of the Roman road.

Air-photographs of the high ridge of Ashdown forest between Hartfield and Maresfield had disclosed portions of an ancient road, in



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straight alignments and apparently of Roman construction, crossing the forest, and the northern portion was found to be pointing directly to Edenbridge. Suspicion that this was actually the continuation of the Edenbridge route was at once aroused, and a search of the intervening country has led to the discovery of abundant traces of the road at many points, sometimes in a perfect state of preservation, and the whole course has been properly established.

Identification of the remains was greatly facilitated by the fact that iron slag (known locally as 'cinder') was very largely used for the metalling for considerable lengths of this part of the road, thus enabling comparatively slight remnants of the agger to be recognized as of artificial origin. By a fortunate coincidence Mr Ernest Straker was just completing his survey of the ancient iron sites of the Weald at this time¹ and the writer is greatly indebted to him for information regarding ancient bloomery sites in the vicinity of the road, and for assistance in investigating and comparing the cinder used in its construction. These ancient bloomeries (marked B on the map) produced cinder identical in type with that used on the road and *may* be of Roman or British origin, though in the absence of definite evidence (*e.g.* pottery) this cannot be proved, for this primitive method was long used. Obviously the huge quantities of slag required for the road must have removed all trace of many of the early bloomery sites.

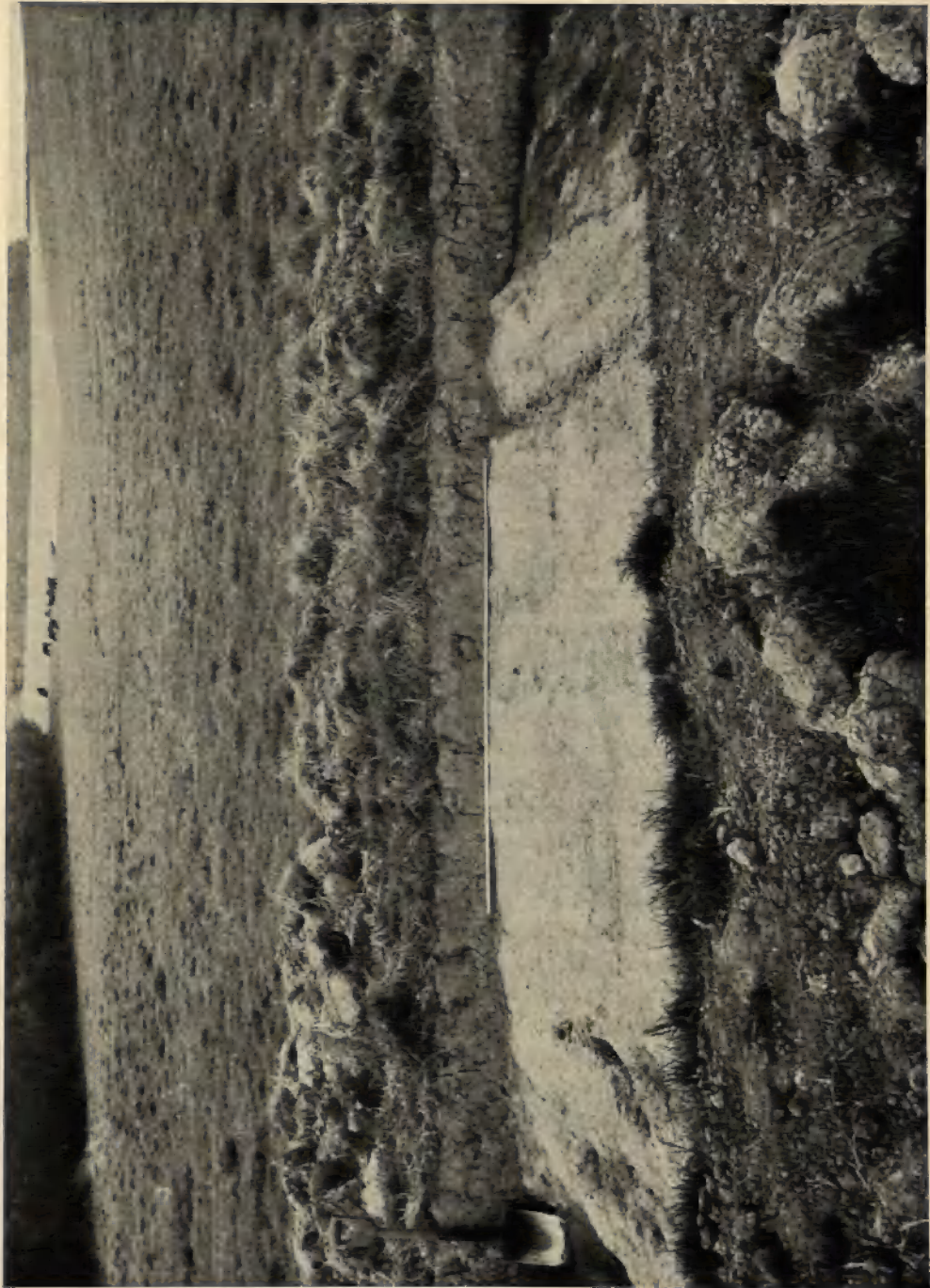
The course of the road will now be described, starting from Edenbridge and working southwards. The existing lane from Den-cross, near Edenbridge, follows the line as far as Cobhambury, a small farm on the Dry Hill ridge, remains of the agger being visible in places.

All trace is then lost through Cobhambury wood but at Beechenwood farm a strip of cinder metalling following an old hedgerow runs from the farm to a small but deep gully that crosses the line. Beyond this the field shows no surface-traces but air-photographs indicate the road clearly as a distinct light-coloured streak leading onward to Ludwells. These traces are in continuation of the Edenbridge line.

Nothing can be seen in the next three fields east of Leighton manor, but in Birchenwood field where the road descends to a gill the cinder is found along the line and a solid bed of it was discovered near the southern corner of the field. For the next 1000 yards the road departs very slightly from the alignment in order to follow a terrace along the east bank of the gill, where the cinder can be readily traced.

¹*Wealden Iron.* G. Bell & Sons, 1931.

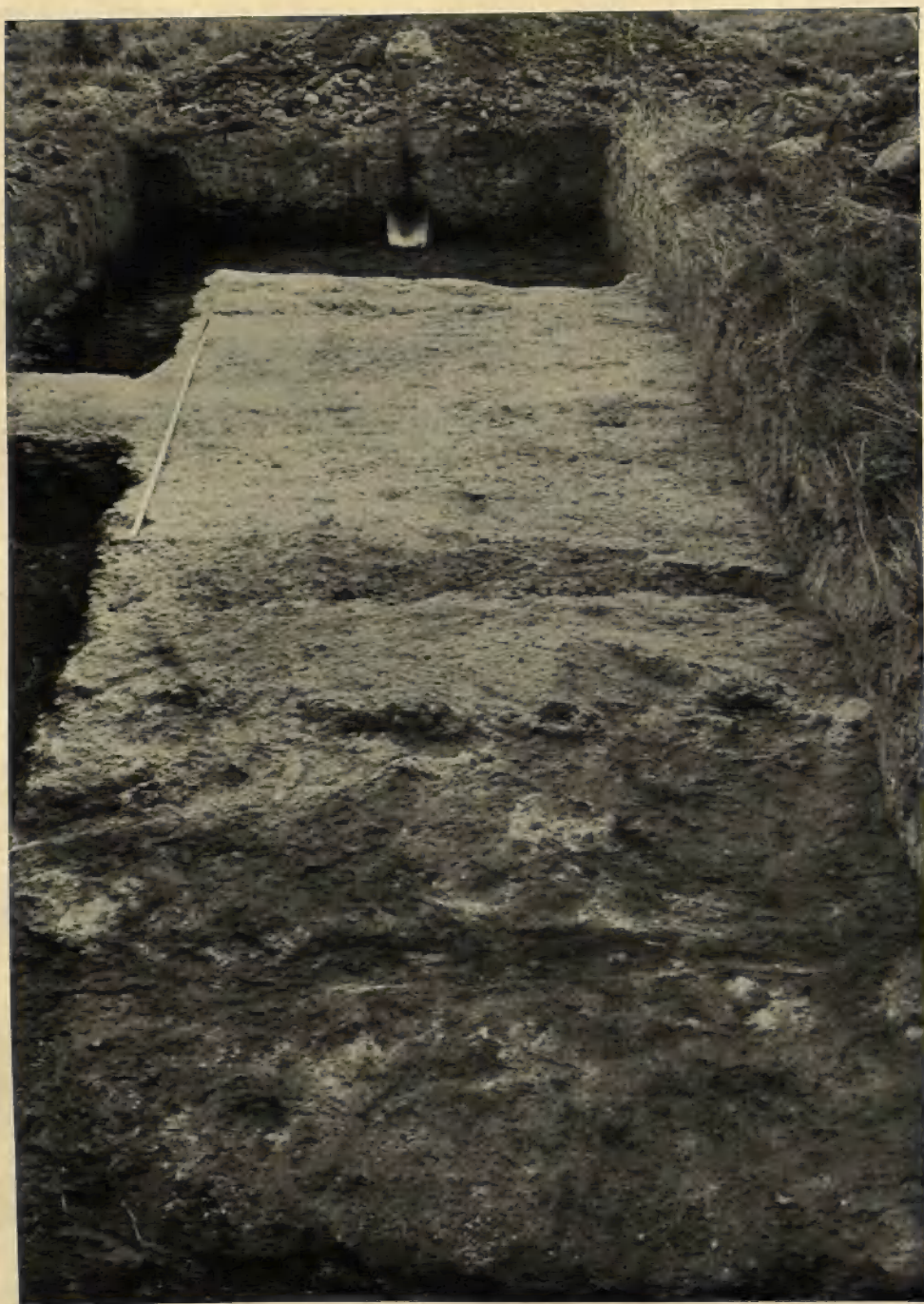
PLATE I



ROMAN ROAD, HOLTVE

Looking north along the line of the road towards Peter's wood. Note the cambered surface and ruts. The spade stands 3 ft. high and the metal rule is 6 ft. long

PLATE II



ROMAN ROAD, HOLTYE

Looking west *across* the road, showing ruts in its surface. The trench on the extreme left was hacked through the concrete to obtain a section and the continuation of the road surface beyond the cut is faintly visible

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It crosses this stream and the Kent Water dividing Kent and Sussex at a point 100 yards west of Kitford Bridge, Cowden, and sections of the cinder metalling are visible in the banks, giving the road an apparent width of 25-35 feet there. It probably lay upon a solid embankment across the valley, but is now buried under 2 feet 6 inches of alluvium.

The agger is distinctly visible as it leaves the valley and traces are to be found in the fields near Holtye House. It becomes very plain through Peter's wood where the perfect surface can be disclosed in places, and crosses the East Grinstead-Tunbridge Wells road 200 yards east of the White Horse inn, Holtye. On approaching the valley to the south of this road the agger is very clear and for a considerable distance is composed of very solid cinder. A section examined near the stream showed the agger in perfect condition, 15 feet wide and 1 foot thick in the centre with a pronounced camber. (See plates I-II). The cinder had rusted into a hard concrete mass which could only be broken with great difficulty. The agger is very plain beyond the stream up the slope of Castle Hill, and though invisible on the surface has been found in good condition at several points beyond in the fields of Bassetts farm.

On approaching the Hartfield-Cansiron lane at Butcher's Cross it becomes visible again, and south of the lane shows perfectly as a fine cambered strip some 50 feet wide across the field. A section here showed the road to have the unusual effective width of 27 feet (35 feet to the edges of the agger), the cinder metalling being as much as 16 inches thick in places. The perfect surface was also found to the south of the ridge on the descent to the river Medway, 500 yards west of Chartners farm, and the river is crossed at a convenient point where the valley is comparatively narrow. The line crosses the Coleman's Hatch-Hartfield road at Gallypot Street, and is indicated by a series of hedge-rows and subsequently by the east side of Posingford wood. From the Sussex boundary to the forest the alignment is closely followed, no part of the road (after the first 350 yards), being more than 100 feet out of line, although several ridges and streams are crossed.

The road enters Ashdown forest at Chuck Hatch, traces of the agger being visible at once. It passes through a small croft and has been much damaged by later tracks. It is traceable up the slope outside the southwest edge of Five Hundred Acre wood and just beyond the crest the road becomes very distinct and perfect. From this point to Camp Hill, across the highest part of the forest, it assumes a very interesting form which is particularly well seen here. The agger is perfect, 18 feet wide with a distinct camber, and is accompanied by

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ditches, 62 feet apart, the intervening side spaces being left flat and unmetalled. This form of construction is exactly similar to that of Stane Street across the highest part of the South Downs where it is accompanied by small ditches identical in shape and character 85 feet apart.² Similar ditches also occur on part of Ackling Dyke. The purpose of the ditches is obscure, for they do not seem intended primarily for drainage, and it may be that they were used to define a roadside zone to be kept free of vegetation or other obstacles when not actually metalled up to the ditches as on Stane Street.

The long alignment from Edenbridge comes to an abrupt and definite end, clearly defined by the ditches, just before it reaches the Groombridge-Maresfield road, and the Roman road makes a sudden turn of 134° southwestward, keeping roughly parallel to the modern road. It then follows the high ridge of the forest past King's Standing to Camp Hill in a series of four main short alignments designed to skirt the valleys sloping first eastward and then westward. Traces of all these alignments remain and have been definitely proved. The ditches show very plainly on the air-photographs owing to the grassy nature of the vegetation contrasting with the heather. Most of the forest section is metalled with local sandstone upon a bed of clay laid on the old heath surface, but near Camp Hill the use of cinder was resumed, this being brought no doubt from the Roman ironworks at Oldlands, only 1½ miles distant.

At Camp Hill the modern road obliterates the track but diverges from it again near Fairwarp church where traces of the agger with its cinder metalling are again visible. Quarrying has obliterated it to the southern edge of the forest, but in the first field of Old Workhouse farm the perfect road surface with hard rusted slag metalling, about 14 feet wide, was found, and in the next field a portion of the agger is distinctly visible as a raised strip (containing the metalling) against the western hedgerow, 100 yards west of the farmhouse.

The road has not as yet been definitely traced through Maresfield park (which has suffered greatly by wartime disturbance), but immediately to the south of the Maresfield-Piltdown road its course is clearly marked, upon the same alignment, by a shallow depression which is also traceable in another field as it approaches a stream. Through the southern part of Park wood, Piltdown, and the western edge of Fairhazel wood beyond, the alignment is further marked by a distinct deposit of

² Curwen, *Sussex Archaeological Collections*, LVII, p. 137 and plates.

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slag now mixed with small brown flints. The line of the road would pass through the bridge at Shortbridge.

In the park of Buckham Hill House there is a very clear piece of the agger, on the west side of a small pond, composed of a hard compact layer of slag about 17 feet wide. It is traceable onwards by means of the scattered slag and flints, closely following the alignment, through Lodge wood and Foxearth wood, emerging close to the southeast corner of the latter.

The exact position of the crossing of the river Ouse is not yet known but it must be almost due west of Isfield church, for only 500 yards farther on the course of the road is again certain, being marked by scattered slag adjacent to a long line of hedgerows past Hole House and Gallops farm. Near the farm part of the undisturbed metalling is exposed by a ditch. It is probable that between the crossings of the Ouse and of a western tributary, Longford stream, part of a drive to Sutton Hall lies upon the road. Apart from the actual crossings the route lies just above flood level.

It is rather surprising that slag was brought so far south, for the nearest source of supply is believed to have been at Maresfield, but it is clear that slag mixed with flints was used right down to a point some 700 yards south of Gallops farm. Beyond this the slag is not found and the road is therefore much more difficult to trace. A definite bed of flints does, however, mark its course with certainty in the south-east corner of a large arable field just before the alignment crosses the railway 600 yards north of Barcombe Mills. It is heading exactly for the crossing of the Ouse at the mill and there can be no reasonable doubt that it recrossed the river there. If this line was followed southward, as is probable, it would graze the large eastward bend of the Ouse near Wellingham (the river has, doubtless, eroded its eastern bank there considerably since Roman times), and follow approximately the southern portion of Wellingham lane, falling into the line of the main Isfield-Lewes road near Stoneham farm. Here it would intersect the line of another Roman road which ran along the foot of the Downs to Glynde and thence over Firle Beacon to Seaford,³ but it certainly appears to have continued straight on to Malling Down, where a turf-covered agger skirting the western shoulder of the Downs is

³A. Hadrian Allcroft. *Arch. Journal*, LXXI, 201. [The Roman origin of this and of many others for which Mr Allcroft claimed a Roman origin, is doubtful. Many of them differ greatly from *aligned* Roman roads.—EDITOR.]

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plainly visible. Perhaps this led down the Ouse valley to the sea, and so avoided the steep ascent by the more easterly route, but it is at least clear that the road connected the iron mines with the coastal downland area.

As for the northern destination it can hardly be other than London, for the line at West Wickham points directly to Rotherhithe only 10 miles distant; this would cut Watling Street at the junction of New Cross road with Pepys road, at the point where Watling Street in its arc from Greenwich to Southwark begins to turn northwestward towards London Bridge, the very place from which a radial road south-south-eastward could best be started. The road may have been planned to serve the dual purpose of connecting the iron mines with the London market and with a Sussex port at the mouth of the Ouse.

IVAN D. MARGARY.

AN IMPORTANT LINK BETWEEN ANCIENT INDIA AND ELAM

No one who is interested in the archaeological discoveries at Mohenjo-daro and Harappa can fail to appreciate the letter of Dr Frankfort to *The Times* (26 March 1932).^{*} The cylinder-seal that he has lately found at Tell Asmar is certainly of Indian workmanship, as shown by the elephant, rhinoceros and the gharial, or fish-eating crocodile, carved upon it. These animals, as is now well known, frequently appear on the seals of the Indus Valley civilization, but in the art of no contemporary culture. Cylinder-seals of Sumerian type are rare at Mohenjo-daro—we have found only two as yet—and none has been found at Harappa; but in view of the trading that clearly went on between India and Sumer it will not be surprising if a good number of seals of this convenient form are eventually found to have been used in ancient India. We should expect to find them at Mohenjo-daro especially, in view of its more easy access by river and sea to Sumer and Elam.

Dr Frankfort has dated his Mesopotamian find to approximately 2500 B.C., as it was found in a house of the time of the Dynasty of Akkad. The question arises: with which of the levels of Mohenjo-daro was it contemporaneous? And here it is noteworthy that in the same buildings as his Indian seal, Dr Frankfort also found a number of heart-shaped pieces of inlay and decorated carnelian beads, which as far as

^{*} See reference in ANTIQUITY, June 1932, p. 227.—EDITOR.



Fragment of Steatite Vase found at Mohenjo-daro with unusual pattern similar to the one below. (c. 2800 B.C.)



Double Vase of Steatite found at Susa. (Period II, c. 2800 B.C.)

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we know at present occur only in the topmost levels of Mohenjo-daro. The two cylinder-seals found at Mohenjo-daro also come from the highest strata, and it seems likely, therefore, that the Indian seal unearthed in Mesopotamia will help us to date the upper levels, which we have before provisionally dated to about 2750 B.C.

We have lately found at Mohenjo-daro an equally interesting and important link between ancient India and Elam; and these two finds in Mesopotamia and the Indus Valley, taken together, should eventually enable us to fix the period of Mohenjo-daro with a considerable degree of certainty.

Our new find was unearthed at a very low level, not far above the water-level in the soil when the river is at its lowest. It is a fragment of a steatite vase bearing exactly the same intricate and very unusual pattern as a double vase of steatite found at Susa in association with other objects of the Second Period of that site. This vase is illustrated by Dr Contenau in his *Manuel d'Archéologie Orientale* (p. 276); and it is reproduced here for comparison with our find. That the vase of which the fragment from Mohenjo-daro formed a part was an importation from Elam is rendered the more certain by its being a greenish-grey steatite, of which it is the only piece that has been found in the Indus Valley excavations.

The dates of the Second Period of Susa are, according to M. de Mecquénem, who is excavating there, and other authorities, *c.* 2800 to 2700 B.C., and it is, therefore, reasonable to regard the stratum in which we found our steatite fragment as of about the same date. If, then, on the evidence of Dr Frankfort's seal, we provisionally date the upper levels of Mohenjo-daro to *c.* 2500 B.C. and we accept *c.* 2800 B.C. as the date of Susa II, and hence of the level of our Elamite find, there would have been an interval of about 300 years between these levels—a conclusion to which I am already inclined on other grounds.

ERNEST MACKAY.

Recent Events

The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.

Our contemporary, the *Bulletin de la Société préhistorique française*, has published an account by M. Viré of the discovery, by means of 'radiotellurie', of a hoard of 15 coins of the 9th century A.D. They were buried in the Salle du Chef Normand in the troglodyte village of Haute Isle. Illustrations of some of the coins and of the jug in which they were found are published. Future developments will be awaited with interest. (*B.S.P.F.*, Feb. 1932, XXIX, 95-7).

o o o

At the Colonial Exhibition held in Paris last year were displayed a number of exhibits illustrating the archaeology of North Africa. An account published in *L'Anthropologie* (XLII, 55-66) complains that the exhibition failed in its object on account of the absence of labels. A map of North Africa in prehistoric times is criticized on the ground that it is rendered illegible by the overcrowding of every period on to a single map. It seems that the force of the slogan 'one map, one period' is still not appreciated by prehistorians. By ignoring it they destroy the value of their own work. What practical value would there be in a map which attempted to show the bus, tram and train routes of London or Paris on the same plan? The account contains trenchant criticisms which reach beyond the actual exhibition itself.

o o o

A French expedition has discovered a human skeleton, fossilized, in Tilemsi near the station of Asselar, in the Sahara, between Hoggar and Senegal. A detailed report, by MM. Boule and Vallois, will appear in *Archives de l'Institut de Paléontologie humaine*. It is claimed to be of Upper Palaeolithic age; if so it is of great significance and will be welcomed by those prehistorians (including the present writer) who regard the Sahara as having been a dispersal-centre of races before it was a desert. (*L'Anthropologie*, XLII, 168; see also Bosch Gimpera, 'La prehistoria africana y el origen de los pueblos camitas', *Anuario de Prehistoria madrileña*, I, 11-28).

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Mr W. F. J. KNIGHT reports :—‘ About half a mile SSE of Bloxham in Oxfordshire seven inhumations, associated with fragments of Romano-British pottery, were discovered during 1929 and 1930. The pottery seems to show that there was a small settlement at or near the site from the early 2nd century until the 4th. The settlement is one of many which have been found, all apparently near rivers, in the basin of the Cherwell. The interments are remarkable: all the skeletons but one lay with their heads roughly to the north, a position not unparallaled (cf. *e.g.* *The Times*, 7 April 1931, p. 12, on interments near Ipswich), and some, probably three, were found buried face downwards, a fashion that seems to be unique at Romano-British sites. (Mr Philip Corder, F.S.A., kindly informs me that the face-downward burials at Wintringham in Yorkshire prove not to be Romano-British). Since my preliminary notice (*Oxfordshire Archaeological Society: report* 74, 1929, pp. 229-32; cf. M. V. Taylor and R. G. Collingwood in *J.R.S.*, 1929, XIX, p. 195), three more skeletons, VIII, IX, and X have been found in 1930 and 1931. Skeletons VIII and IX seem to have been buried in a single trench, as in Romano-British graves at Hook Norton (Margaret Dickins, *A History of Hook Norton*, p. 3). The arms of no. VIII were folded across the chest, and the bones of the forearms are unusually thick. A report on all the skeletons is expected. Skeleton X was found later, eastwards from VIII and IX. Boys of Bloxham school have helped in the recovery of the remains, and have carried out some excavation’.



A Society to be known as the Friends of the Palestine Museums (Palestine Art-Collections Society) has been formed, its interests extending ‘to all objects illustrating the history and antiquities of Palestine and the development of the various movements which have their roots in that country’. Among the activities of the Society will be a survey of the material which does exist, the publication of catalogues which will make it more widely known, and the formation of a permanent exhibition in Palestine. The headquarters of the Society are 9 Cavendish Square, London, W. 1.



A shrine of the Nabatean period is reported from Jebel Rumm, 25 miles east of Akaba, Transjordan. It contains inscriptions, one mentioning Rabel, the last ruler of the dynasty (c. A.D. 71-100). *The Times*, 17 June).

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Some important carved ivories have been found in Samaria, where excavations have been resumed by the Joint Expedition at work there. These are representations of Ra, the Sun-god, holding a figure of Maat the goddess of Truth; Horus, Isis, and Nephthis, and Hah the personification of Eternity. It is thought that the ivories are the work of the same school as those found at Arslan Tash, near Carchemish, in 1928. (*The Times*, 16 June).

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The discovery of the human skull found in 1931 by Mr C. Ter Haar in a bone bed at Ngandong, in the valley of the river Solo, Java, is referred to by Professor Elliot Smith in a letter to *The Times*, 3 June, and a detailed account by Sir Arthur Keith was printed in *The Illustrated London News*, 4 June.

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Owing to the drought in Switzerland this summer some of the lakes have fallen to very low level and interesting observations have been possible on the site of the lake-dwellings at Corcelette, near Grandson, on the lake of Neuchâtel. The site comprises Stone and Bronze Age types. Numerous finds of swords, axes, flint implements and other articles have been placed in the Lausanne Museum. (*The Times*, 10 June).

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The discovery by Mr Theodore MacCown of the skeletons in the Valley of the Caves at Mount Carmel in Palestine (reported briefly in *The Times*, 4 May, and *Nature*, 14 May) is described in some detail by Miss Dorothy Garrod, Director of the Joint Expedition of the British School of Archaeology in Jerusalem and the American School of Prehistoric Research, in *The Times*, 11 June, and *The Illustrated London News*, 9 July. In the latter there is also an article by Sir Arthur Keith on the importance of the discovery of this newly found type of Man (*Palaeanthropus Palestinus*).

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M. Schaeffer, the well-known excavator and discoverer of the Ras Shamra tablets (see ANTIQUITY, 1930, IV, 460-6) is coming to London in October at the invitation of the University, to deliver three lectures. They will be given at King's College on 17, 20 and 21 of October at 5.30 p.m., and will be illustrated by lantern-slides and a film. Admission is free to all.

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The subscribers to the Fund for establishing a British School of Archaeology in Iraq, as a memorial to Gertrude Bell, who died in Baghdad on 12 July 1926, have now appointed a Council on which the Universities of Oxford, Cambridge, Edinburgh, Durham, London, the British Museum, the British Academy, Royal Geographical Society, Royal Asiatic Society, Society of Antiquaries and other Societies are represented. The President is Sir Percy Cox (late High Commissioner for Iraq), the Chairman of the Executive Committee is Sir Edgar Bonham Carter, Mr E. H. Keeling is Honorary Secretary and Brigadier-General Sir Osborne Mance Honorary Treasurer. The funds raised up to the present total nearly £9,000. In addition, the income from sums of £6,000 and £4,000 bequeathed by Gertrude Bell and by her father Sir Hugh Bell to the British Museum on trust will, it is hoped, be payable to the School. The disposal of the income is under consideration. The office of the School is temporarily at 20 Wilton street, S.W. 1.



We have been asked to announce the appearance of two publications likely to appeal to readers of *ANTIQUITY*. One is a map of 'Neolithic Wessex' published by the Director-General of the Ordnance Survey, Southampton. It is on the scale of 4 miles to an inch, and shows the long barrows, circles, inhabited sites and flint mines which fall within the area of Sheet 11 of the quarter-inch O.S. map. Physical features are indicated by layers; the primitive forests have been restored upon a geological basis; and the modern details of roads and villages are printed in a light grey, to assist identification. Rivers are in blue, and marshland is shown in the same colour. The archaeological items are numbered with reference to a schedule which, together with some explanatory text, is bound up in the same cover. The price is 4s (flat paper copies without text 2s).

The other is a pamphlet of 34 pages called 'Field Archaeology: some notes for beginners', issued by the Ordnance Survey (6d). It can be obtained direct from the Ordnance Survey Office, Southampton, or any of the local agents, or from H.M. Stationery Office, Adastral House, Kingsway, W.C. 2. It gives a simple description of the main features of certain 'common objects of the country', such as barrows (long and round), circles (of earth, wood and stone), standing stones, camps, Roman villas, castle mounds, cultivation terraces, trackways, linear earthworks (such as Offa's dyke and the Grim's ditches). Each section has a concise bibliography of books and articles suitable for further study. The first impression is already nearly exhausted.

Reviews

SAILING SHIPS; their history and development as illustrated by the collection of ship-models in the Science Museum. Part I. Historical Notes. By G. S. LAIRD CLOWES. *Board of Education*, 1931. pp. 114, with 38 plates and 11 text-figures. 3s.

The great increase in the models of boats and ships at South Kensington and their scientific re-arrangement in the new galleries have rendered out of date the guide compiled by Sir George Holmes many years ago, and the first part of a descriptive handbook by the present Keeper is therefore most welcome. A brief note on the origin of the collection is followed by a summary of the sources of information by which the evolution of the sailing ship can be traced. It is emphasized that we are fortunate in still being able to see and handle many kinds of craft which represent stages in the development of the wooden ship, for the more backward races of mankind continue to employ their primitive artefacts; the Australian aboriginal still crosses a stream astride of a log pointed at both ends; certain Indian peoples navigate their rivers on two pots lashed up with bamboos; reed-bundle canoes and dug-outs have even today a very wide distribution. The main theme being the evolution of the modern wooden sailing ship, there is first a detailed survey of the Eastern Mediterranean and its tributary waters, for here were built the early ships which many centuries later came to perfection after the far younger ship-culture of the Atlantic coasts came into continuous contact with them during the 14th century. The development from sea-going ships to such vessels as H.M.S. *Victory* and the China tea-clippers has proceeded without sudden changes in hull-construction and sail-plan. We are first told what is known of the ships of Mesopotamia, Phoenicia and the Aegean, and of the ancient Nilotic craft: the ship-cultures of all these countries contributed to that of classical Greece, whence was derived the larger and more seaworthy ship of the Roman domination.

Of the ships of Phoenicia and their descendants of Carthage it is unfortunately impossible to say much in view of the only direct representations being some coins and *graffiti*, but we would have welcomed more than the few lines given towards reconstructing the ships which ventured so far. The next chapter takes us to the North, with a reference to the *graffiti* which survive on the rocks of southern Sweden and other parts of Scandinavia and are usually ascribed to the Bronze Age, and which have been the subject of so much speculation, even to their not being boats at all but sledges, or even outrigger canoes, a kind of craft found nowhere in Europe.

In the burial-mound ships, commencing with the Nydam example of about A.D. 220, and those of the Viking Age found at Oseberg, Gokstad, and elsewhere, we possess a fund of information. Of these Mr Laird Clowes gives a summary which will enable the reader to appreciate clearly the essential differences in construction which separate them from Mediterranean ships, and to realize that the double-ended open craft which we call a 'whale-boat' and its relatives among fishing craft of Northern Europe are the lineal descendants of these Viking rowing-boats which set a square sail in favouring winds.

The English one-masted ship of the 11th to the 14th century owes much to the beautiful lines and proportions of the early northern ships, as we learn from the seals of certain coast towns, illustrations in MSS, and on painted glass, which show that English

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and Baltic ships of the 12th and 13th centuries were essentially northern, the clinker-build as opposed to the carvel-build of the South being one of their outstanding features. The advent of the three-masted ship has very properly a chapter to itself, and the transition period of two masts, the question whether the early second mast of the North was stepped forward or aft of the 'great mast', and other cognate matter are summarized.

From this transition period, during which southern devices and fashions were developing their influence, we come to the reigns of Henry VII and VIII, distinguished by marked advance in size and improvements in hull and rigging. In ship design the reigns of Elizabeth and the Stuarts brought many improvements rather than fundamental changes. The 17th century, in Mr Laird Clowes' words, was 'a period in which, for the first time in the history of ships, we are able to speak with certainty of the exact dimensions of individual ships, of their precise appearance, and of the details of their fittings'. Towards this knowledge the builders' models of Stuart days are of inestimable service to us. The 'Establishments' of the early 18th century introduced that great experiment in standardization with a rigidity which, though often disregarded, made us fall behind the French with their wider range of vision and three schools of ship-design when we maintained none. The Royal Navy had to fall back on copying prizes, but at last hard and bitter experience and the employment of better seasoned timber gave us the ships by which the great victories of the late 18th century were won. The last fifty years of the sailing ship and the evolution and decline of the clipper ship lead us through the main changes in hull and rig which culminated in such triumphs of naval architecture as our *Duke of Wellington* and the *Bretagne* of the French Navy in the 'fifties of last century, the tea and wool clippers of the next two decades, and the last deep-sea ship which can be called a type, the four-masted barque of the 'eighties.

Mr Laird Clowes does not forget that he is writing for the inexpert but interested visitor to the models, and his choice of illustrations is excellent. Here and there we would venture to suggest revision in a future edition. The vexed question of the origin of the lateen sail is hardly yet on a basis for the statement (p. 13) 'that there is every reason to suppose that the basal idea of a fore-and-aft sail was originally derived from the Pacific islanders'. That surviving inventories show that English ships carried guns as early as 1410-12 should be mentioned alongside the statement (p. 60) that guns were first used on shipboard in 1461. The seal of Louis de Bourbon of 1466 is given as the first dated representation of a northern three-master, in neglect of the miniature in a manuscript possessed by Lord Hastings and ascribed to not later than 1450; this portrays several three-masted ships of northern type. As the earliest record of a true stern-post rudder slung by pintles and gudgeons we are shown the seal of Elbing of 1242, but an equally modern-looking rudder is seen on the seal of Ipswich which there are grounds for believing was contemporary with the charter granted in 1200. This very early English example deserves mention. The numerous illustrations are excellent. Part II (published 1932, 2s 6d) of the handbook consists of a catalogue of the models, with very full descriptive notes.

H. H. BRINDLEY.

LES SILEX DE BUDEL. By J. BUTTER. Amsterdam: Buys & Zoon. pp. 19 and 12 pages of plans and line drawings. Price not stated.

Before M. Butter began his researches no traces of Tardenoisian culture had been recorded from the Netherlands, a circumstance the more curious when we recall the magnificent work of Rahir and Lequeux in Belgium. It is satisfactory to know that

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industries of this most interesting culture are being recovered from yet another area. In the present pamphlet two stations are described, both situated on land of the factory of the Société Anonyme des Zincs de la Campine, Brabant. The first of these appears to be of Neolithic date, having yielded a polished axe, leaf-arrowheads, a tanged arrow-head, and a few sherds of pottery showing fine quartzite mixture. It is interesting to note the presence on this floor of a considerable series of trapezes. The second station, situated at a kilometre's distance from the first, proved to be a chipping site. It lay immediately below the heather on sand. The most typical forms met with were non-geometric microlithic points, blunted down the whole of one side or else obliquely down part of one side, usually the left. There also occurred rather rarely types usually associated with the final Tardenoisian, as found at Zonhoven (upper level), including a trapeze, a concave-based triangular point, and a point with pressure-flaking. It is probable that these may be disregarded in assessing the industry as a whole, since, like the leaf arrowhead also found, they probably represent strays. A feature of the industry is the presence of the burin of upper Palaeolithic type (mostly of the angle variety) in very considerable numbers. The microburin is not mentioned either as occurring or as absent. This is a pity, as it provides a most useful index. A number of points of gravette character, in some cases well over two inches long, together with a broken point of 'pen-knife' type, combine with the burins to suggest an upper Palaeolithic facies. Indeed M. Butter concludes that his second station shows a culture transitional from the final Magdalenian to the Mesolithic having some morphological relations with the final Aurignacian. The industry taken as a whole is certainly not of an evolved Tardenoisian character, though we regard it as definitely Mesolithic in age. How far it was influenced by the Tardenoisian at all it is difficult to say, since we are given no information on the type fossil of that culture. It seems probable that at any rate the basis of the Budel microlithic culture must be regarded as a development from the northern fringe of upper Palaeolithic culture. In this country we can show certain parallels from Lincolnshire in the West Keal and Sheffield Hill sites, and in the lowest level from Scunthorpe.

J. G. D. CLARK.

ROCK-PAINTINGS OF NORTH-WEST CÓRDOBA. By G. A. GARDNER, *with the collaboration of S. E. GARDNER.* Oxford: Clarendon Press, 1931. pp. xvi, 147, with 172 text-figures, 44 plates (28 in colour), and 3 maps. 84s.

Here is a book which cannot fail to interest the mind and rejoice the eye of anyone who cares for prehistory and the beginnings of art. It is an album of rock-paintings, recorded with the utmost accuracy obtainable by the use of camera, brush, pen and tracing-paper, guided by a scientific mind, and admirably reproduced in colour plates, half-tones and line-drawings. Whatever fresh light the years may bring—and one cannot hope for much—to the interpretation of the material, this collection of pictographs and petroglyphs is bound to remain a standard work of reference in its own field. The illustrations are mainly the work of Mrs Gardner, and both authors are to be heartily congratulated on a really fine achievement, which was also a labour of love carried out at no small personal sacrifice.

Over six years of intensive field-work were devoted to the study and copying of the paintings, which are situated in rock-shelters of the Córdoba hills in the Argentine. No systematic work had previously been attempted here, and only a few of the paintings had been copied before by Leopoldo Lugones, and reproduced by Félix F. Outes in his

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monograph on the archaeology of Córdoba. One feels therefore that, in turning the pages of this handsome volume, one is being admitted to an entirely new province of primitive art, and one of very great interest. Its isolation lends it a peculiar value for the comparative student. For there can be no question here of influences or connexions with the better known cave-art of Europe, or of the South African Bushman.

It is all the more interesting to find the artistic instinct manifesting itself on parallel lines, in a technical sense, with that of the Bushman, *viz.*: in the form of incised or 'pecked' engravings on isolated boulders, and of paintings on the walls and roofs of rock-shelters. The natural environment was not dissimilar in the two cases. But the state of culture of the respective artists was very different. Unlike the Bushman, who was a hunter pure and simple, the Comechingons of Córdoba, to whom these paintings must be ascribed, were essentially a settled agricultural folk, and many of the figures depict domesticated animals such as the llama. For sheer naturalism and variety the hunters, at their best, must, we think, still be accorded pride of place. But the Córdoba artists attained to a high degree of realistic expression, and many of the figures have great individual beauty. There is also a large number of geometric and inexplicable symbols, to intrigue our interest and challenge speculation.

Unfortunately the Comechingons had been practically exterminated within 150 years of the first arrival of the Spaniards; so that no final and authoritative solution of the picture-puzzles is perhaps possible today.

In date the majority of the paintings are almost certainly pre-Hispanic, that is, earlier than the 16th century; superpositions, the state of preservation and other factors indicate a period of many centuries during which the art flourished before this. A limited number of figures of Europeans and equestrian figures proves that it was still alive, if decadent, at the time of Cabrera's expedition in 1572.

No definite sequence of styles or colour can be deduced from the superpositions of paintings, which do not follow any consistent arrangement. But the author gets the general impression that the geometric figures, which are generally red, mainly preceded those of animals, for which white is the colour more usually employed. In style the paintings are classified into six groups. Analyzed by subjects they fall naturally into three categories, *viz.*:—natural objects (men, animals, birds, insects, etc.), geometric designs (some of which may be astronomical, *e.g.*, the rayed circles), and incomprehensible figures. Nearly 70 per cent. belong to the first class, the remaining 30 per cent. being fairly evenly divided between the latter two groups. White, black, red and grey are the colours employed, and the figures are usually in monochrome, white being the commonest colour. There is a variety of techniques, among which one may note the 'spotting' method, and, rarely, a 'negative' style, in which the design is 'reserved' in the natural colour of the rock and the background painted in.

The old problem of significance is impartially considered in chapter vi. No positive conclusion is reached, but in the cases where the paintings are placed in awkward and inaccessible positions, high up or under low ledges of rock, a magical or religious motive seems necessary to explain them. Some groups of figures, on the other hand, would seem to be commemorative of historical events. Mr Gardner wisely refrains from dogmatic verdicts. On one point only is he convinced—that they were done with a definite object and not merely to pass the time idly away. Nevertheless one may be permitted to wonder whether a purely artistic impulse, such as we know to have been deeply rooted in many branches of the American Indian race, was not seeking its natural expression without *arrière pensée* in at least some of these attractive pictures.

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Chapters I-III contain a detailed description of the sites and paintings; chapters IV-VI an analysis of style, technique, date, origin and significance. A very useful appendix of comparative material, a bibliography and an index complete this admirable volume.
H. J. BRAUNHOLTZ.

WANDERINGS IN GREECE. By F. S. BURNELL. *Edward Arnold*, 1931. pp. 253 and 22 illustrations. 7s 6d.

This is a completely satisfactory handbook, learned, light and interesting. The author's practical information is up-to-date, his facts of travel sound, his style unwearying. He is in touch with the latest archaeological controversies and makes many of the show sights of Greece the more interesting by enveloping them in the mysterious clouds of learned dispute. Nor does he merely tell the reader of the all-too-well-known marvels at the expense of the minor wonders. For even on the most trodden tracks there are so many things that the ordinary guide forgets. How many tourists who tramp up the Acropolis or round Kerameikos look at the church of St. Demetrius the Bombardier or climb up the Mound of Sulla? How few see the lovely spring of Kanathos near Nauplia, whose magic properties are now wasted on the sisters of a convent? No tourist to Greece should miss this excellent book.
S. CASSON.

SCULPTURED PORTRAITS OF GREEK STATESMEN, WITH A SPECIAL STUDY OF ALEXANDER THE GREAT. By ELMER G. SUHR. *Baltimore: Johns Hopkins Press; London: Humphrey Milford*, 1931. pp. 189 and 23 figures. 24s.

Almost all the extant ancient portraits which are not Roman copies are of men unknown or little known. Euthydemus of Bactria and Kotys of Thrace, men hardly known to history, survive in superb portrait-heads. But the great statesmen and the philosophers and generals have almost all come down to us in poor, sometimes appallingly bad, copies. To disentangle the identities of these copies of copies of portraits, to distinguish exactly how many versions of the head of Aeschylus or Euripides or Plato were made, is a task which can appeal only to an academic mind and an undaunted spirit.

Dr Elmer Suhr is possessed of both these qualities. His book is in effect the thesis which gave him his doctorate, and in its ingenuous outlook, its persistence in the pursuit of knowledge, and its unflagging study of the least interesting *minutiae*, it is characteristic of all such theses.

Frankly he has no new contribution of any kind to make to the present state of iconography, and in some respects he is even out of date, as, for instance, in his belief that Mausolus and Artemisia stood upon their chariot, or in his statement that clay models for stone-carving came in as late as Pasiteles (actually Pheidias and Paeonius must have used them).

But his book will be of some considerable use as a work of reference, for he has accumulated the facts with untiring diligence.

His pages are marred more by simplemindedness than by error. It is, for instance, startling (if gratifying) to read that the hideous apotropaic dance-masks from Sparta 'probably do not represent statesmen', or to hear that 'Professor Robinson . . . and some other students of Macedonia now believe that a sort of Greek civilization started in Macedonia at least as early as 2000 B.C.'. No one would dispute these truisms, but few would venture to assert them thus!
S. CASSON.

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THE ROYAL TOMBS AT DENDRA NEAR MIDEA. By AXEL W. PERSSON.
*Lund : C. W. K. Gleerup ; London : Humphrey Milford, 1931. pp. 152, 86 figures
and 36 plates (4 in colour). 42s.*

The results of these exceedingly important excavations of tombs on the site of the only major settlement of Mycenaean Argolis which has hitherto remained unexcavated contribute more to our knowledge of the Mycenaean mainland than any excavations of the last twenty years.

Dr Persson has indeed been fortunate. By finding an unrobbed Tholos tomb containing the burial of a king, a queen and a princess he has found more than the most sanguine excavator of the much dug Argolid could normally dream of discovering. But the very cause which had prevented robbers from seizing the treasure here described almost equally successfully operated in preventing Dr Persson from finding it also. For both the Swedish excavators and ancient treasure hunters who explored it as early as the 11th century B.C. had alike entered the Tholos tomb and found it empty. But Dr Persson, unlike his predecessors, was not to be baulked, and by digging beneath the plaster floor of the tomb discovered the burials where they had been so wisely hidden. He admits, however, that he was on the point of concluding the excavation of the empty tomb without searching beneath the plaster floor, had not the natural thoroughness of a trained archaeologist compelled him to examine the floor down to rock level.

The wealth of gold, silver and bronze vessels and ornaments found mark the burial as in every sense royal. The date of the latest burial, that of the king and queen, is fixed by the ceramic evidence at the first half of the 14th century B.C. But the excavators think that the tomb was built during the king's lifetime, for his use, and that the princess was buried first.

Out of so much wealth it is difficult to say which is the finest or the most important work of art, but it seems wisest to award the palm to the superb gold cup, already one of the most famous of Cretan works of art, found with the king's skeleton. The cup held by the queen, on the other hand, is less beautiful though of no less importance. Its design of bulls' heads in niello and gold and its mixture of metals—a gold interior and silver exterior—make this artistically unique and, from the point of view of metallurgy, of the deepest interest. There seems no question that it is Mainland work and not an importation from Crete. An ostrich-egg cup, found between the king and queen; a large cup like those from Vaphio and with a similar embossed design of bulls, but differing from them in being of gold inside and of silver outside; a silver goblet decorated with a hunting scene and some superb gems form the main prizes of the rest of the treasure.

Among the minor finds the most interesting are a group of finger-rings made of layers of mixed metals—silver, lead, copper and iron—which the author tentatively suggests were of magical value because of the electric current which the association of these metals can produce. Two leaden horns from a helmet and some particularly striking paste plaques also from helmets throw no little light on the panoply of the Mycenaean warrior.

In addition to the Royal tomb there were excavated a series of chamber-tombs, and the excavator's luck held here also. In one was found an enormous deposit of bronze vessels, consisting of no less than 35 objects in all. Another chamber-tomb proved to be a cenotaph, which the excavators think is perhaps that of a warrior who was killed in one of the mainland raids on Egypt in the 13th century.

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Dr Persson is to be congratulated on a careful and most adequate publication of material which is as vital to our knowledge of the Mycenaean world as it is to our study of Homer. His apt references to the text of Homer show how much valuable illustration this excavation can afford to Homeric scholars.

It is perhaps to be the more regretted that some parts of the book are extremely hypothetical. The author's complete rejection without argument of Evans's recently expressed views on the Tholos tombs of Mycenae leads him to adopt alternative views about the mainland Tholoi which are no more and no less satisfactory.

His interpretation of two glass plaques as Europa and the Bull and Bellerophon and the Chimaera is extremely hazardous, and his argument from this interpretation that these myths can now be given a pre-Hellenic origin is consequently very risky. The plaques are so corroded that any firm identification of their design is speculative in the extreme.

The author is perhaps also a little too definite in his assumption of a Mainland expedition from the Argolid to Crete which was responsible for the destruction of Cretan power and the looting of Cretan wealth. There is much likelihood that something of the kind happened but no shred of evidence to prove that it did, and to argue from this speculative hypothesis that the 'Octopus Cup' and many other clearly Cretan works of art in the Tholos tomb were loot brought back by the king of Midea is not the sort of argument which should appear in a scientific publication of this kind.

Apart from these flights of fancy the publication of these amazing finds is sound and of the deepest interest. The colour plates are admirable and the photographs in the main adequate.

S. CASSON.

A CLUE TO THE CRETAN SCRIPTS. By F. MELIAN STAWELL. *Bell & Sons*, 1931. pp. 120, and 11 figures. 15s.

Attempts to decipher unknown scripts can only be of two kinds—those which are aided by hints from other and known contemporary languages, and those which use only the internal evidence of the scripts themselves. This attempt is of the latter type, and the second type of decipherment must always, in a sense, be *in vacuo*. For since there has not yet been found any single Minoan word written in any transcribable script of pre-Greek times Miss Stawell is driven unavoidably to guess at the language, which must for the moment be called Minoan. That language may be entirely unknown; on the other hand it may have affinity with some language spoken in the Aegean of which we have some knowledge. Miss Stawell boldly assumes that the language used for the Minoan scripts and pictographs was an early and simple form of Greek. This startling assumption she defends with well-reasoned and not wholly untenable arguments. She presumes a state of affairs in the Aegean in which Greek of a sort was spoken as early as 3000 B.C., and that it had come in with the Minoans from the northeast; these 'Minoan' newcomers acted as a leaven upon the primitive inhabitants of Crete and the Aegean and produced the marvels of Cretan art. In other words she projects into a remote past a movement similar to that which introduced later the Achaeans and Dorian dialects of Greece.

Once granted that the basis of the scripts is Greek the rest is easy. She translates the texts as fluently as her predecessors—Hempl and Kluge—in this unfruitful field. For, once you admit that a certain sign resembles a certain object and take several Greek words for the object which it seems to represent you can play about indefinitely with

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alternatives until you get some sort of translation. The method simply cannot fail, and I have no doubt that if it were equally assumed that the basic language of the Minoan tablets and seals were German or Latin or Russian, that equally good or bad translations would ensue. In recent years the learned attempts made to establish that the opening lines of every Greek play contain in anagram a 'signature', a 'chronogram' and an 'ascription', shows how a skilful scholar can juggle with his material. The ribald soon showed that you can get almost any anagram you like out of the opening lines of the plays, and one ingenious scholar even proved in this way, as a *reductio ad absurdum*, that the first lines of the 'Oedipus Tyrannus' of Sophocles stated that the author of the play was Euripides! So much for decipherment *in vacuo*.

But the archaeological facts are against Miss Stawell's assumption. There is no archaeological context for 'Greeks' in the Aegean that can even with faint plausibility be placed earlier than 2000 B.C., and, even so, that context was far up in the Balkans.

But, once granted her Greek, Miss Stawell forges ahead and only when she identifies the actual seal-stone of Minos himself with his name in plain blunt characters will the acquiescent enthusiast feel that it is too good to be true.

I must confess that much of the introductory matter is sound. We are tempted to see too much un-Greek Minoan and too little Greek in these dark millennia before the Achaeans came. Miss Stawell shows, plausibly enough, that we are not fully justified in presuming a non-Greek and pre-Greek language which has survived into Greek with its -ssos and -nthos endings. While Fick's analysis of this evidence is sound enough, his assumption that it implies a pre-Greek language is after all but a hypothesis. But, against Miss Stawell, it can be shown that it is a hypothesis that works, while her hypothesis of early Greek in the Aegean will not accord with the archaeological evidence. Nor is she for a moment correct in saying that 'when the Minoan marvels were first discovered all scholars were struck by their likeness to the works of later Greece'. Scholars then, as now, saw mainly that a complete gulf of artistic outlook separated the two modes of art. Minoan art implied a certain outlook on life and artistic creation which was fundamentally different from that of Greece. Nor can any evidence be found satisfactorily to bridge that gulf. Minoan art implies a certain personality of the artists which predicates a different racial origin. The resemblances between the two modes of art are simply the resemblances which are inevitable when two vigorous and independent peoples are actively artistic in the same region—and, of course, certain methods of the earlier people were transmitted to the later if only because the whole Minoan race was not exterminated in the dark age that heralded the Iron Age into the Aegean. Miss Stawell will not get her Greek into the Minoan world that way.

But all that clear and persevering scholarship can do is done in this book, which is an intelligent and painstaking attempt to do the impossible.

S. CASSON.

DIE FORM DER PALÄOLITHISCHEN GERÄTE. By F. HERIG and G. KRAFT.
Archiv für Anthropologie, band XXII, 1932.

Of recent years several prehistorians have been undertaking a new kind of investigation of stone implements. This involves the careful measurement of the different types of tools occurring in the various cultures—length, breadth, thickness, weight, etc. It is hoped that as a result a new classification will be found possible. Friedrich Herig is an engineer who has brought his exact training to bear in this way on the Magdalenian industry found at Petersfels, and he has produced an impressive piece of work. He

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classifies all stone tools into 4 groups : gravers, borers, scrapers, coups de poing or daggers, since these are each used in different ways, and require different positions of the hand to hold them. Thus today we hold a pencil differently to an awl, and a scraper differently to a dagger. It will be noted that the author is even more interested in the holding-edge of a tool than in the actual working-edge—a point of view which perhaps can lead to error since the hand is a very adaptable organ. Using this fourfold classification the Petersfels industry is carefully examined. Also the proportion in which each type occurs is noted. Naturally little immediate result seems to follow; what is needed is that similar work should be done in the case of other finds both of the same and different ages, and then perhaps useful comparisons will become possible.

G. Kraft continues with a general article on the shapes of palaeolithic tools. He discusses form and material, secondary working and function; also pure form. He seems to see in the shaping of stone tools of different ages not merely utilitarian reasons but also the growth of true aesthetic feeling in Mankind.

These special studies of prehistoric tools are still in their infancy; much more work will be necessary at other sites before it can be affirmed that really important results accrue. Such articles as the above, therefore, are to be welcomed. M. C. BURKITT.

KULTURSTROMUNGEN IN EUROPA ZUR STEINZEIT. By G. ROSENBERG.
Copenhagen : Andr. Fred. Host & Son, 1931. pp. 176, and 357 figures. 5 kr.

It may be said at once that no student of early British pottery can afford to be without Rosenberg's study of the origin and spread of cord and thread impression as a means of ceramic decoration. One may go further by commending the work to anyone interested in the phenomena of the genesis and development of decorative technique in whatever field. For Rosenberg has unfolded his evidence in such a way as to give us a most intimate insight into the processes at work in the formation of a special technique and its multifarious varieties. Here, as in so many cases, the strictly rational explanation of a cultural phenomenon has proved to be the false one. The theory which sought to explain the cord and thread ornamentation of pottery as the result of using cords and threads for the support of vessels when drying, while as a matter of fact it has certain physical objections, has received a certain amount of support from those easily fascinated by explanations of a deterministic character. The Ussatova find, however, has confirmed Sophus Müller's view that cord and thread ornamentation must be connected with women's needlework. It was not a physical accident but a human whim that gave rise to the new system of ceramic decoration. The two sherds illustrated by figs. 34 and 36 show beyond all doubt the impressions of neck-bands of plaited threads, which can be easily reconstituted (figs. 35, 37). Clearly the woman potter has placed her own neck-band around the neck of her pot, the child of her hands. It is amusing to note in this connexion that a clay idol from Tripolje (38) has a neck-band depicted by twisted thread impressions.

If the impression of these neck-bands inaugurated a new system of ceramic decoration, it is equally certain that it inspired the earliest motives of the new style. Thus in the links of the double plaited chain (35) we see the prototype of the crescentic 'maggots' made by impressing a loop of twisted thread with a finger of the right hand, while the tassels of 37 explain the decorative fringe to be seen on such sherds as 39. A special form of pendent decoration, consisting of a line of arcs or scallops, which occurs rarely in South Russia (74, 139), but commonly in East Prussia and Denmark (274-6 and 311-5),

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was probably inspired in a similar manner. As usual, however, the origin of the decoration was soon forgotten and even at Ussatova itself, the cradle of the style, we find single elements employed in the formations of new patterns regardless of their earlier significance. In the same way we note a tendency for the zone of decoration to extend from a narrow band round the neck of the pot until eventually the whole pot is covered. Both these processes become accentuated as the style becomes more removed from its source in time and space. Thus, already at Aleschki-Kinburg near the mouth of the Dnieper, we note the marked popularity of the 'winkel' pattern, formed by two or more rows of oblique 'maggot' impressions, each row forming an obtuse angle with its neighbour (85). We also note the introduction of a hard core in place of the finger for impressing the thread, causing straight in place of crescentic 'maggots' (89) though the more primitive method extended over the whole area of diffusion to the British Isles. At Pluti, near Kiev, more than 500 miles upstream, the 'winkel' pattern was also much favoured, though here we note that it was commonly executed with the edge of a mussel shell, either plain (148-53) or serrated (154-5). Moreover, as sherd 163 demonstrates, the potters of Pluti sometimes carried their decorative patterns over the whole surface of the pot down to the very base. Perhaps the most interesting point to be noted, however, is the use of the comb (154-5) in the execution of a pattern originating in the cord and thread area of South Russia, and also the association of pits and 'maggots' on the same sherd (142-3). This points to the intimate relationship which existed between the thread and cord province of South Russia and the great comb pottery province of the North. At Ussatova itself we find perforation decoration on the same sherd as crescentic 'maggots' of the earliest form (69). It is of domestic interest to point out that in Britain we find the same pit-motive so characteristic of comb ware on pottery of the Neolithic period of the Peterborough class, on which 'maggot' is very frequent, e.g., the Wandsworth pot sherds from Peterborough (*Archaeologia*, LXIX, figs. 9-10), a sherd from the Thames (London Museum, A 13667), and sherds from the West Kennet long barrow (nos. 77 and 80 of Mrs Cunningham's compilation). The system of ceramic decoration which thus developed in the Ukraine spread up the Dnieper to the East Baltic coastlands and Finland on the one hand, and via East Prussia, Bornholm and South Sweden to Denmark, finally to reach the British Isles. The connexion of our Peterborough ware with the Baltic was first noticed by Mr Reginald A. Smith, and has more recently been amplified by Professor Childe in a notable communication to the Royal Archaeological Institute. If readers of *ANTIQUITY* wish to appreciate an important class of the Neolithic pottery of their own country they cannot do better than buy Rosenberg's inexpensive work. It is hoped that with the aid of the brief summary here presented and the excellent photographs available in the book those without German may be able to obtain most of the important information they require. If one may be permitted to make one criticism it is that there is no map. The inclusion of a good map, indicating the type-sites and judiciously hachured, would we think have brought into relief the amazing area of diffusion of the thread and cord system, stretching as it does from the Kuban to the British Isles and from Bessarabia to Finland. It might further have been utilized to indicate graphically the culture-contacts made by the province along its peripheries. A greater appreciation of space-distribution as a factor of equal importance with time-distribution would have spared him a certain puzzlement over the relation of his group to the painted pottery of the Black Earth region. At Ussatova itself the arrival of painted pottery appears to have post-dated the earliest phase of the thread impressed wares, while at Kiev the thread impressed pottery antedates almost

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completely the arrival of the painted ware. Rosenberg is puzzled that at Cucuteni the thread impressed ware is stratigraphically later than the earlier phases of the painted pottery. We can only point out that, whereas at Ussatova and Kiev we have to deal with peripheral intrusions of painted ware into the heart of the thread impressed zone, at Cucuteni we have to envisage an exactly opposite state of affairs. That the time-relation of the two wares should differ as between, for instance, Kiev and Cucuteni, is only to be expected. All of which goes to show that stratigraphy by itself is a very dangerous method establishing a time-relationship between two cultures, unless it is checked by a study of geographical distribution. Time-sequence, in fact, can only be established by a study of distribution in both its vertical *and* its horizontal aspects.

J. G. D. CLARK.

ROMAN BRITAIN. By R. G. COLLINGWOOD. *Oxford: Clarendon Press, 1932.* pp. xii, 160, with 59 figures and map. 6s.

The text of this book originally appeared in the *World's Manual* series. It has now been carefully rewritten and amplified, gaining in limpidity and force and becoming considerably enriched by new ideas. Among illustrations, the 52 blocks of the older book have been replaced by 59, of which 25 are quite new and a vast improvement upon those which they replace. Others, again, have been redrawn and enlarged, and all are printed upon much better paper. Readers new to the subject will also be grateful for the amplified bibliographical appendix. No one who possesses the older version need think that it will suffice in comparison with the new: for the text has been changed in tone as much as the illustrations; and if we were gladdened by the original work, how much more may we not bid this one welcome and good speed.

I. A. RICHMOND.

THE ROMANS IN BRITAIN: an anthology of inscriptions with translations and a running commentary. By A. R. BURN. *Oxford: Basil Blackwell.* pp. 228, with 4 plates. 6s.

This admirably-written textbook will be of the greatest use to schoolboys and other young students for whom it is meant, since it will teach them much and inspire them to learn more; proceeding, we hope, from Britain to the Empire at large. Criticism confines itself to minor points but, in view of the second edition which we hope the book may have, we mention the following. The Claudian triumphal inscription is now in Palazzo Barberini, Rome. *Speculatores* of Legion II cannot be taken in relation to the course of its earliest campaigns with safety. The Chichester inscription is not from Goodwood Park, but now happens to be *in* it. The Castleshaw tile reads COHIIIBRE and connects with Slack, not Manchester; nor is it related to successive occupation of the two places. Among successful naval operations on the West Coast please mention Demetrius's adventurous voyage. It is high time that this got into our textbooks, for while Demetrius was a teacher, no one says that he taught in Britain, and we *do* know that he accompanied an Imperial Expedition of Inquiry and Exploration to the Scottish Isles; further, if the York inscriptions be his, the vow to Oceanus and Thetis should refer to that adventure (see Dessau, *Hermes*, XLVI, 156), not to the humdrum channel-crossing. Again, in connexion with York, Trajan's twelfth tribunician power does not run on into A.D. 110. In the same period, Mr Burn's account of the Trajanic disaster is over-precise, while that of the building of Hadrian's Wall might well be amplified. Finally, is there any proof that the Derbyshire mines were administered from Anavio?

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But these are relatively small matters, and we welcome the book and commend it to all sixth-forms and to many others who are beyond that stage. To their bibliography, however, we beg readers to add the annual summary of Romano-British discoveries of the *Journal of Roman Studies*, which ANTIQUITY does not attempt to emulate.

I. A. RICHMOND.

THE ACROPOLIS. *Photographed by* WALTER HEGE, *described by* GERHARDT RODENWALDT. Oxford: Basil Blackwell. pp. 61, with 37 text-figures and 104 plates. 37s 6d.

Many picture books of places and things merely irritate by their text and misinterpret by their illustrations, which, all too often, illustrate only the virtuosity of the photographer. Nothing can lie so blandly as a camera if the intention of the photographer is the merely picturesque or impressionistic. But recent developments of photography under the influence of modern filmwork has at last convinced photographers that the camera must no longer be considered merely as a rival to the artist. It has a technique and a sphere that belong to it and to nothing else.

Herr Hege has as fully realized this, as has Dr Rodenwaldt that a text should illuminate in the manner of literature, and not serve merely to enable the reader's eye to seek rest from drama in pious commentary. I know of no book where the balance is so well kept between the illustrations and the reading matter. After Dr Rodenwaldt's brilliant introduction—which might well serve as a model introduction for all time to a work on Greek art—one accepts the lovely photographs as part and parcel of a combined study of a great shrine by two men who come as near to understanding what that shrine meant to ancient Greeks as it is possible for modern men to do. Dr Rodenwaldt's critical and explanatory descriptions of the Parthenon frieze, of the Propylaea and of the Erechtheum give the non-specialist reader an immediate insight into the heart of the subject. His simple but sensitive account exceeds in merit all the conventional textbook descriptions of the archaeologists' handbooks. He has the merit of brevity and comprehension not only of the achievements but also of the aims of the artists of Periclean Athens. And that makes the much over-written monuments of the Acropolis take on a new lease of life—a service long needed, for they have for too long been tarnished by volumes of mediocre appreciation and unwanted description. Here for the usual word-pictures of the Professors we have the photographs of Herr Hege, which speak for themselves. What Herr Hege cannot provide Dr Rodenwaldt gives us.

So many recent photographs of the Acropolis have been taken solely for the purpose of dramatic effect. We must admit at once that the Acropolis cannot help being dramatic from some points of view. Certainly the Greeks intended it to be so. But what is dramatic to us today was not drama to them and the aspects which we so often get in modern photographs of the Acropolis would have gone counter to their taste. Herr Hege's finest work is on quiet themes that reveal Athens in its normal mood. Nos. 3 and 5 show us the Acropolis from a distance without the *bravura* of a Dodwell engraving, and free of the thunderstorm background so dear to the picture-postcard maker. The camera in these views is working honestly as a camera and not as a drop scene at the Lyceum. No. 16 shows us not only a lovely view along the peristyle of the Parthenon but also gives us simply and without emphasis a view of the main refinement of the building—the curvature of the main floor. No. 22 is an admirable view of one of the metopes now in place, a feat of difficult photography, for how it was taken I do

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not know. So too with no. 25, a view of the frieze. No. 73, the Nike temple frieze, is in the same class. All the plates reproduce the clarity and precision of the architecture and sculpture.

A few misprints that have crept into the translation call for revision. 'Peisistratides', 'Mycænian' and, more serious a fault, 'Aegean' (for Aeginetan) must be eliminated in a new edition. The only disappointment is the choice of the slabs of the Nike balustrade. Herr Hege has omitted the most beautiful.

But these are very small defects. The book is a wholly admirable production and one which every lecture-ridden tourist to Greece and every classical scholar should possess, if only to serve as an antidote to the heavy matter that he has for so long absorbed. S. CASSON.

NORDISK KULTUR. Vol. ix. A. Folkevisor. *Edited by Prof. KNUT LIESTØL.*
B. Folksagner och folksagor. *Edited by Dr C. W. v. SYDOW.* pp. 294 and 8 plates.

Volume xxvii. Kunst. *Edited by HAAKON SHETELIG.* Stockholm: *Albert Bonniers*; Oslo: *H. Aschehoug*; Copenhagen: *J. H. Schultz.* pp. 466, numerous illustrations. Prices not stated.

The two splendid volumes before us form a small part of a cooperative work on Nordic Culture, running into some 30 volumes. Every phase of the life of the northern peoples of Denmark, Norway, Sweden, Finland and Iceland, their habitat, physical characteristics, social structure, and material and spiritual culture, is covered by the series, each volume of which is edited by a Scandinavian scholar of distinction. Furthermore each separate contribution is made by an acknowledged expert under the general direction of the editor for the volume. By this method the whole ground is covered without serious overlaps or omissions and in a thoroughly authoritative manner. The illustrations are numerous and good; paper and printing are first-rate. We should be grateful to the Clara Lachmanns Fund which alone made such a work practicable. The Scandinavian countries may be industrially 'poor', but culturally speaking, having regard to their resources, their performance far outstrips that of many richer and more powerful states.

The volume on Art is divided into three parts: prehistoric, medieval and folk, of which the first is the longest. In the first J. Bøe gives an account of the naturalistic art of the Stone Age as seen in rock-engravings, paintings and sculptured figures; C. A. Nordman deals with the handwork of the Stone Age as revealed in pottery and flint and stone implements, and later with Nordic ornament in the Finnish Iron Age; J. Brøndsted deals with the decorative patterns and forms of the Bronze Age, as well as with the art of the Celtic and Roman period and the art style of the Migration Period; Gunnar Ekholm gives a good account of the famous rock-engravings of the Bronze Age; the Early Viking period is treated by Jan Petersen and the Later by Sune Lindquist, while Prof. Shetelig himself contributes an article on foreign influences in the Scandinavian art of the Iron Age. Medieval architecture, sculpture, wall-painting and textile fabrics of Denmark, Norway, Sweden, Finland and Iceland are described separately. Finally the furniture, utensils and textiles of the Folk are dealt with. The Folk culture of Scandinavia is still strong in remote areas, and its influence is seen among the sophisticated population of the cities, contributing a traditional flavour even to the finest achievements of modern art such as the city hall of Stockholm. Visitors to the recent exhibition

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of Swedish art in London will remember the happy marriage of the modern and the very old that is so pleasing a feature of contemporary art in Scandinavia. The volume on folk-songs and stories does not directly concern readers of *ANTIQUITY*.

J. G. D. CLARK.

THE ALPHABET: ITS RISE AND DEVELOPMENT FROM THE SINAI INSCRIPTIONS. By MARTIN SPRENGLING. *University of Chicago Press, 1931.* pp. 71, 4 plates. \$1.

There have been many studies of the strange alphabetic writing which occurs on the rocks at the temple of Serabit in Sinai. The most ambitious was by Grimme, who imparted a meaning to the natural cracks, and produced sensational readings about Moses and Hatshepsut. Cowley came to the conclusion that many lines referred to a body of Arab workmen called after the Negeb region. Gardiner and Leibovitch looked on them more cautiously. Now Prof. Sprengling has made a fresh study, and gives consistent readings throughout the series of sixteen scrawls, mostly fragmentary. He recognizes truly that the original script must have been cursive pen work, by its curved forms. Curiously, though most readers give *tnt* for a group on a bust, and on other stones, they have not proposed to take this as Tanit, the name of the Punic goddess who was identified with Astarte, therefore=Hathor the goddess of this temple. The soft sandstone weathers and breaks readily, and the condition of this writing in most instances leaves only short groups.

The translator's progress leads him to launch into a 'Story of the Alphabet', attempting to show how the Phoenician was developed from the Sinai script. Not half the signs can agree with this, and he has overlooked that nearly all the Phoenician signs were already in use in Egypt as far back as the 1st dynasty, part of the larger series of the Mediterranean signary. The Sinai writing is probably a variant of the Phoenician series, and apparently an invention of the Sinaitic workmen. FLINDERS PETRIE.

MEDIEVAL SCULPTURE IN FRANCE. By ARTHUR GARDNER, F.S.A. *Cambridge University Press, 1931.* pp. xix, 492, with 498 illustrations, 113 plates. 73s 6d.

As the author of this magnificent book states in his preface, he is concerned with medieval sculpture in France rather than with French sculpture, and so his work has arbitrary geographical limitations; but to the ordinary traveller who is obliged to confine his voyaging to one or another country at a time, this is an added convenience and recommendation.

The book deals shortly, but sufficiently, with the historical events that led to the great developments in medieval sculpture, to the Renaissance which culminated in the magnificent Romanesque work of the 11th and 12th centuries, to the growing fineness of the transitional work leading to the glory of the 13th century, and the change from architectural to more purely sculptors' sculpture during the Flamboyant period. These sections are of great general interest, but the main part of the text is taken up with a consideration of the characteristics of the different regional schools of sculpture, and with a detailed description of the sculpture itself. Here the author shows a sureness of identification, and a conversance with the style and individual characteristics of the work that can only have come from the most intimate and minute study of the originals.

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When it is added that the index of places contains some 380 names, and that in many cases several buildings were visited at the same place, the amount of devoted labour that has gone to the compilation of the book can be imagined. In spite of the mass of detail that it contains the easy style and occasional humorous touches make it very readable.

The origins of much of the earlier figure-sculpture in ivories, metal-work and, above all, illuminated manuscripts is very fully brought out; and as we follow Mr Gardner through the five great centres of Romanesque building in the south of France—Languedoc, Burgundy, Aquitaine, the Auvergne and Provence—we cannot help feeling that his heart is more bound up in them even than in the great cathedrals of the north. And we shall be the last to blame him. It is difficult to think of any French church containing important sculpture which is omitted from either the text or the supplementary lists at the end of the chapters.

The book is illustrated throughout by photographs. Most of them are from the author's own camera, and they are generally excellent, while their reproduction leaves nothing to be desired. Naturally, all are not of equal value, but all photographers know how difficult it is when visiting places for a short time to obtain conditions that are most favourable for different situations. If it had been possible to vary the illustrations by the use of a few line drawings it might have helped occasionally in the elucidation of certain points. But there is little to cavil at in this great volume, and the reader is left feeling how much he has missed in the past, and how much greater in the future will be his appreciation of this most human art.

DINA PORTWAY DOBSON

DIE GERMANISCHEN GRIFFZUNGENSCHWERTER (RÖMISCH-GERMANISCHE FORSCHUNGEN, band 5). By ERNST SPROCKHOFF. *Berlin: Walter de Gruyter, 1931. pp. 117 and 32 plates.*

The wide distribution of the flange-hilted sword is the most arresting feature of the late Bronze Age in Europe, and the origin and significance of the type have attracted the attention of many archaeologists. Its apparent rapid spread, from Britain to Greece and from Spain to Sweden, has contributed not a little to the difficulty of establishing its place of origin. Competent British students have given the peoples of central Europe the credit for its invention, but it may be recalled that Naue and Müller favoured Italy whereas Undset favoured Egypt as its land of origin. More recently, Reinecke has argued for an Aegean origin, while Remouchamps has even sought aid from the Hittites. Still another school has used the sword in defence of the 'Germanic' (West Baltic) culture-province. The present monograph is a careful and detailed study of the flange-hilted swords of that important area.

It must be said at once that Sprockhoff is no quixotic champion of Germanic independence. He is, it is true, impressed with the importance of the West Baltic province, and complains that Naue did not appreciate its wealth; but these are proper sentiments for the regional worker to hold. Though he believes with Kossinna that the rivetless flange-hilted sword is a product of 'Germanic' inventiveness—and most will agree that it is a very early type—he is not concerned to fight about origins. His work, in fact, is a study of regional classification, distribution and relative chronology.

Although flange-hilted cut-and-thrust swords are surprisingly uniform in general plan from one end of Europe to the other, no detailed system of classification yet proposed

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has met with universal approval: Sprockhoff advances still another. He places the swords of his province into three groups in chronological sequence—older, common and younger varieties—and subdivides each group typologically. One feels grateful to him for avoiding the use of alphabetical labels to give his types an artificial air of permanence.

The older swords are two in type: the familiar one with curved rivetless tang and rounded shoulders, and the rarer straight-tanged form with sloping shoulders: the former is claimed to be indigenous whereas the latter is admittedly central European. They are both dated to Montelius II (Reinecke c). Of the common types of sword (Naue II) the Germanic province has yielded over 350 examples, mostly of Montelius III (Reinecke D). A fixed date for this period is provided by the well-known sword of Seti II (c. 1200). Some would place the period on the whole before, some after, this date. Sprockhoff, following Müller, is inclined to lower the absolute chronology, *i.e.*, he would commence the common-sword phase towards 1200. One reason for this lies in his conviction that period IV is neither so long nor so important as Montelius estimated. It is interesting to observe that this criticism parallels that made for British chronology by Dr Cyril Fox.

A narrow tang is the main distinguishing mark of the younger (*i.e.*, later) bronze swords. The shoulders are usually rounded once more (a sign of old age this time) and the blade is broad and long, with a bold mid-rib outlined by parallel lines. This sword is derived from the common form under the influence of South Germany. It has certain features in common with British swords of the type of Beachy Head, an example of which is given from Hannover. Other instances of bronzes of British or North French workmanship found in North Germany are cited. Swords of central European origin are considered in a brief section, and this leads on to an account of Hallstatt forms in bronze and iron.

Next follows an exhaustive inventory of every variety of flange-hilted sword found in the west-Baltic province: there are over 700 entries. But of still greater value are the distribution maps, six in number, very clearly executed and wisely confining themselves to single sword-types. On the basis of these excellent maps much interesting work remains to be done: the text does little more than draw attention to the sharply limited extent of the culture-province and to the way in which it spread southeast between Elbe and Oder as the Bronze Age advanced. The 25 plates of swords and associated objects also deserve our warmest praise.

One point that emerges clearly is the rarity of the 'leaf-shaped' or expanding blade in Germanic swords of the flange-hilted variety. It is time this confusing label was abandoned. To the reviewer, Sprockhoff appears to place too much reliance on pure typology: for example in his allocation of swords with slotted hafts or with hilt-projections to special types. But it must be remembered that he is dealing with a restricted area, and we could heartily wish for more regional monographs as thorough as this.

ESTYN EVANS.

ART IN THE LIFE OF MANKIND: a Survey of its Achievements from the earliest Times. III. Greek Art and its Influence. IV. Roman Art and its Influence. By ALLEN W. SEABY. *Batsford*. pp. VIII, 110; VIII, 110, and illustrations. 5s each.

This volume on Greek Art is written from rather a new point of view. The author possesses, besides a sound historical background, technical artistic knowledge, which enables him to view the art of the Greeks from the standpoint of the craftsman as well

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as the connoisseur. As a result the various problems of artistic development are treated with a simplicity and reality that is specially suitable in a small and elementary, and at the same time, extremely comprehensive work. Besides the inevitable subjects such as Greek architecture, sculpture and vases, the book contains useful sections devoted to jewellery, cameos and coinage which add materially to its usefulness. The illustrations are excellent.

In his *Roman Art* Professor Seaby has contrived to set forth a vast amount of information. Beginning with pre-Roman art in Italy he touches on the work of the Villanovans and Etruscans, mentions the more characteristic Roman buildings and smaller objects, includes a few notes on Roman Britain, and then sets out to trace the influence of Roman Art and the art handed on by means of the Roman organization, taken in its widest sense, to the younger peoples of the West. The Sassanian dynasty is included, and also Coptic art, and so we are led to the stone crosses of Northumbria.

It is doubtful whether the genesis of these northern patterns is as simple as the author would have us believe, but in a small volume there is not much space for speculation, and it is a pleasure to come upon a book written so definitely from the evolutionary standpoint. The illustrations are numerous, and are composed of photographs and many beautiful and informative sketches by the author.

On page 6 the Second Crusade is made responsible for the sin of the Fourth, in sacking Constantinople, but such slips are rare, and the book should be very useful to all who are interested in the genesis of Western Art. DINA PORTWAY DOBSON.

THE HISTORY AND ANTIQUITIES OF THE SCUNTHORPE AND FRODINGHAM DISTRICT. By HAROLD E. DUDLEY with chapters by GEORGE R. WALSHAW. *Scunthorpe: W. H. and C. H. Caldicott, 1931. pp. xvi, 247, with 64 illustrations. 7s 6d.*

This book is the first attempt to collect all the information about the interesting region in the northwest corner of Lincolnshire round the new iron-smelting town of Scunthorpe since Andrew published his *History of Winterton* in 1836. Seventy-one pages are devoted to archaeology and the rest to a series of chapters on local life in medieval and modern times, including a description of the remarkable rise of Scunthorpe from a hamlet to a town of 30,000 inhabitants since the discovery of the ironstone in the 'sixties.

The most important feature of the prehistory of the district is the occurrence of the richest pygmy flint-site in England on the sandy warren to the east of Scunthorpe town. Mr A. Leslie Armstrong regards this as the type station of the Tardenoisian in England. The site continued to be occupied through all the succeeding periods down to the close of the Roman occupation, and after the Tardenoisian settlement the most interesting belongs to the early Bronze Age, which has left numerous hearth-sites on the peaty floor of the Warren, with much beaker pottery, many flints, and a few of the bronze implements peculiar to this phase of the Bronze Age. The whole of the Bronze Age is well represented in the finds which have been made round Scunthorpe, and the occupation of this neighbourhood was far denser than that of any other part of the county at this time. The reason for this is not far to seek, for the dry, lofty situation overlooking the marshes of the Trent and close to the Humber, one of the great water gates of eastern Britain, makes it ideal for settlement.

Mr Dudley deals with the Iron Age, which is not well represented round Scunthorpe

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any more than in the rest of Lincolnshire. Now that some intensive work is being done on the archaeology of the county Iron Age relics are beginning to turn up more frequently, but they remain uncommon, and that Scunthorpe should have so little to show is remarkable, since it lies at the north end of the Lincoln Edge, which has been held by Dr Cyril Fox to have been the probable main line of communication at this time between the East Yorkshire region and the South and West of England via the oolite ridges.

It is in the Roman period that the Scunthorpe area again proves rich in finds. These, as well as many others of earlier and later date, are due to the complete removal of all the surface soil which takes place in the iron mines. The method is not ideal for scientific excavation, but it reveals everything, even though the objects fall from the hopper of a steam excavator as it swings round to deposit its load in the waiting trucks. The thickest concentration of Roman material has been found along the base of the cliff from below Winterton Hill to Thealby, and the most interesting fact which emerges from the study of these remains is that some iron smelting here in Roman times seems certain.

At Scunthorpe this work was carried on near Bagmoor, but scoriae probably belonging to the Roman period have been found as far south as the neighbourhood of Scawby several miles away.

The rest of the book contains a record of the district's medieval and modern history, to which Mr Walshaw has made some valuable contributions. The process of the rise of a new industrial community out in the country in the later 19th century makes instructive if cautionary reading.

Mr Dudley has wisely confined himself to presenting a record of the facts about the Scunthorpe district and has refrained from making more than tentative deductions from them. It is clear that there is much more to be found, and his caution is to be commended. The book is very successful within its chosen sphere, and breaks the long silence which has prevailed over the archaeology and topography of Lincolnshire.

One serious defect exists—there is no map, and while this would be a regrettable feature in any book dealing with a district, it is especially so here where there is much detailed description of topography.

The illustrations are good, but is not the sword from Flixborough shown on page 114 medieval rather than 17th century in date?

C. W. PHILLIPS.

A CATALOGUE OF THE GREEK VASES IN THE ROYAL ONTARIO MUSEUM OF ARCHAEOLOGY, TORONTO. By DAVID M. ROBINSON, Professor of Archaeology in the Johns Hopkins University, and the late CORNELIA G. HARCUM, formerly keeper of the classical collection in the Royal Ontario Museum. *Vol. I, texts and drawings, pp. vi, 288; vol. II, plates. Toronto University Press, 1930.*

This is a descriptive catalogue of the Greek vases, and the Etruscan or Italic vases showing Greek influence, contained in the fine collection of the Ontario Museum. The mere cataloguing and classification is a good piece of work, which required years of careful attention; but the great value of the book lies in the careful and accurate descriptions, for the illustration of which Professor Robinson has visited the chief museums in Europe and America in search of parallels. Mr J. H. Iliffe, the present keeper, has edited the book as a whole, and added an appendix of recent acquisitions, and proposes to keep it up-to-date by notes from time to time in the Museum *Bulletin*. The second volume contains 118 excellent plates, every figure in which has numbers referring to the catalogue, and to the accessions list. The advantage, to the reader, of having the plates separate is obvious.

J. F. DOBSON.

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EXCAVATIONS AT KISH : Oxford Field-Museum Expedition. Vol. III. 1925-7. By L. CH. WATELIN, with notes and selection of contracts by S. LANGDON. *Paris*: Geuthner, 1930. pp. 20, plates I-XVI.

THE FIELD MUSEUM—OXFORD UNIVERSITY EXPEDITION TO KISH, MESOPOTAMIA, 1923-29. By HENRY FIELD. Leaflet 28 of the Field Museum of Natural History. *Chicago*, 1929. pp. 34, plates I-XIV.

THE EXCAVATIONS AT KISH. [Report of a Lecture by Prof. S. LANGDON given to an unnamed Society after the seventh season's work. Reprint, pp. 291-300].

EXCAVATIONS AT KISH 1928-9. [Report of a Lecture by Prof. S. LANGDON, given to the Royal Asiatic Society, 12 Dec. 1929. Reprint from *J.R.A.S.*, 1930, pp. 601-10, with plates VII-XI].

The ancient sites at Kish must be almost as difficult to reconstruct as is the narrative of the excavation. Vol. I left off in the middle of an account of the Sumerian Palace on 'Mound A', and of the buildings on 'Mound w'. Vol. II is not yet published. Vol. III describes the large Neo-Babylonian temple Ehursagkalamma on the mound called Ingharra, which resembles temples already known at Babylon and Borsippa. This temple was constructed with complete disregard of older monuments adjacent, and was under repair when the kingdom of Nabonidus fell. It yielded very few portable objects, and no inscriptions except on the bricks. As earlier remains have been discovered beneath the massive brick platform on which it stands, it is to be entirely destroyed, and 'will be preserved for future generations only in the pages and plates of this volume'. The inscriptions published in the same volume do not seem to have anything to do with it.

From Professor Langdon's lectures we learn that he thinks he has terra-cotta figures of horses as early as 3500-3000 B.C., and 'the word for horse' in Sumerian pictographic tablets from Jemdet-Nasr as early as 4000 B.C. (p. 295). He dates his great 'flood' deposit about 3300-3000 B.C., but though it devastated Kish it did not reach Jemdet-Nasr, 17 miles away, owing to the greater concavity of the valley in those days (p. 296). The predominantly longheaded people from graves about 3000-2800 B.C. are thought to succeed to predominantly broadheaded about 4000 B.C.: but even the earliest graves hitherto found are of mixed folk. It is not clear what he means by saying (p. 298) that 'the Sumerians perished utterly at the end of the third millennium B.C.', and he gives an odd account (p. 299, 610) of a terra-cotta head (figured *JRAI*, 1930, 2, ix, 5) from one of the later levels. By assiduous pumping, excavation has now reached virgin soil, six feet below the earliest 'painted-ware', which was preceded by 'only stone and flint implements'.

In *JRAS*, 1930, pl. VII, we have at least a diagrammatic section by Mr Penniman of early deposits under the temple Hursag Kalamma; it emphasizes the contrast of cultures before and after the 'flood' deposit, which is here dated 3400-3200 B.C. The 'red-stratum' immediately above it looks like a deliberate attempt to raise the site above such risks from flood.

From these fragmentary jottings we turn to W. Field's quite popular but intelligible summary of the whole enterprise, with photographs of the early chariot-wheels, the rein-ring surmounted (as at Ur) by a well modelled ass, the fine copper 'rush-light' or cresset, the vigorous victory-relief, and typical tablets and pottery. Some idea is given of the methods of excavation, with rather large numbers of workmen, and rather

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small expert staff, too often replaced by novices. The man on the spot must of course use his discretion ; but the reviewer at home is inclined to ask why it was necessary to remove at vast expense the whole of the later monuments, to reach the earlier levels. Did not any part of the antediluvian city lie clear of the later platform ? J. L. MYRES.

LES ORIGINES DU TITRE DE SAINT-MARTIN AUX MONTS A ROME.

By RENÉ VIELLIARD. (Studi di antichità cristiana publ. per cura del Pontificio Istituto di Archeologia cristiana, vol. iv). Rome ; Paris, 1931. pp. 133. 40 lire.

The Christian community of Rome was, at an early date, divided into parishes called titles (*tituli*), which were named at first after the founder of the church, often a layman who had given or built the house which became the meeting-place of the congregation. At the beginning of the fifth century there were twenty-five titles, but many of them dated back from a much earlier time, sometimes as early as the third century. In a few cases the original meeting-house still remains buried in the crypt of the present church or hidden behind some later buildings. But a comparative study of written documents (such as the *Liber Pontificalis*) and archaeological remains may throw a new light on both the primitive churches of Rome and the administrative history of the Christian community.

In a monograph written under the supervision of the Pontifical Institute of Christian Archaeology, M. Vielliard has achieved this purpose for one of the most interesting of the titular churches, St. Martin in Montibus. His conclusions can be briefly summarized here. The title was founded in the early third century by one called Equitius, probably under Pope Callistus (217-22), at any rate in the time of the Severi, and at a date contemporary with the buildings of San Clemente, Santa Anastasia and SS. Giovanni e Paulo. Whereas in the other three churches the Christians simply bought an ordinary Roman house and reconditioned it in order to have, on the first floor, a big meeting hall (*salle liturgique*), at St. Martin, on the contrary, it seems as if they built the house on purpose : the ground-floor was almost entirely occupied by a hall, large enough to accommodate a congregation of 200 ; the court-yard was thus thrown to one side, with the staircase leading to the cellar ; at the main entrance was a short triangular vestibule, and another staircase leading to the two upper-floors, where the clergy had their living quarters.

In the beginning of the sixth century, the house, which was then known as *titulus Silvestris*, and had become a popular place of devotion to the memory of Pope Silvester, was too small to house an increasing congregation ; but it was left practically untouched by Pope Symmachus who preferred to build, next to it, a large basilica dedicated to St. Martin. Alterations were made in the course of the 9th century, and later in the 13th, when Franciscan monks took charge of the building and remodelled it for housing their convent. But, if massive pillars, new walls and arches have done much to disfigure the original house and puzzle the archaeologist, the main walls and the primitive vaulting are still there, and Rome can claim, according to M. Vielliard, ' to have thus, almost untouched, one of her third-century churches '. E. PERROY.

WATERS OF ARUN. By the late A. HADRIAN ALLCROFT. Methuen, 1930. pp. xiv, 170, with 6 illustrations and 21 maps. 7s 6d.

This is an interesting book on one of the most fascinating of southern rivers, written in the easy, pleasant, almost 'chatty', style one has learnt to expect when the author wrote about Sussex.

He starts from an ancient road which, running through a part of Arundel Park,

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where there is evidence of Romano-British occupation, goes straight down to the river. Here he sought and found a shelf 10 or 12 feet wide and about six feet high on the upper side crossing the bed of the river at right angles and in line with the ancient road.

Convinced that this must be an artificially made Roman causeway, and recognizing that at its present depth—3 feet below water at the lowest tides and 15 at the highest—it could not have been a practicable crossing, he sets out in ten chapters to assemble all the evidence he can find to prove that the Arun is not really the dwindled estuary of 'the literary tradition', but rather a stream in which tidal waters have been steadily gaining and the channel deepening since Roman times.

The quest takes us through much pleasant country; over downland and meadow, along bypaths and old parish boundaries, over lost roads and forgotten fords, by dredged channels and navigation cuts, to derelict quaysides and old harbours, over long 'fulls' of shingle and stretches of ancient 'havens', past the new mouth of later commerce to the old outlet miles to the east and even out to sea over miles of ancient Sussex now submerged. All this is discussed with an extraordinary wealth of knowledge: ancient charters, manorial records, ecclesiastical sites, feudal changes—every sidelight of local history is brought to bear on the problem, and with never a tedious line.

The book is not of course all serious, still less rigid, archaeology. The author's narrative style makes little distinction between his proved facts and his inferences and he never pauses to marshal the one or to weigh the other. He pleads his case with charming advocacy and makes no pretence at an impartial summing up. But as a companionable and scholarly guide through a delightful country with all the interest of an unsolved problem to lure us on the book could hardly be bettered.

'Waters of Arun' is published as a memorial volume by the Brighton and Hove Archaeological Club. A list of the author's publications is given consisting of four books, headed by *Earthwork in England* in 1908, to which all field-archaeologists owe an abiding debt as the first and only classic on the whole subject, and 20 papers contributed to divers journals, mostly on Sussex archaeology. It would be a great boon if these stimulating papers which form the basis on which he built so much of the lighter structure of his 'Downland Pathways' could be republished in one collection.

All who knew the author and his work will appreciate Dr Cecil Curwen's In Memoriam notice.
J. P. WILLIAMS-FREEMAN.

DER BREITE BERG BEI STRIEGAU; eine Burgwall-untersuchung. *Bearbeitet von G. BERSU, H. KURTZ, E. PETERSEN und H. SEGER.* (Schlesischer Altertumsverein in Breslau). Teil I. Die Gräbungen, by G. BERSU. *Grass, Barth & Co., Herrenstr. 20, Breslau.* pp. 46 with 38 illustrations and 4 plates in colour. 24 marks.

This is an elaborate account of excavations carried out on the site of the hill-fort on the hill known as Der Breite Berg. The hill was fortified first in Hallstatt times; though these fortifications were of no great strength the site seems to have been occupied for a considerable time, and foundation-posts of rectangular houses of the period were found. This occupation seems to have ended in burning and catastrophe, and the numerous triangular bronze arrowheads found are thought to have been missiles used by the enemy in their attack upon the place. The hill was then abandoned until it was very strongly fortified by Slavonic people. The excavations seem to have been carried out with great care and thoroughness, and many interesting details of the Slavonic fortifications were obtained.

REVIEWS

In conformity with the shape of the hill the area entrenched was roughly triangular ; the point of this triangle was strengthened by wooden towers, but the base line has been destroyed by modern stone quarrying, so the main entrance or gateway was not available for examination. On both sides of the hill just within the entrenchment, and roughly parallel with it, were foundations of rows of wooden rectangular houses of the Slavonic period.

The plans and drawings illustrating the work are excellent, but the photographs of sections are not as good as they might be. The finds seem to have been numerous, especially for the second, or Slavonic period, but these are to be dealt with separately in a later publication.

M. E. CUNNINGTON.

KULTURGESCHICHTLICHE WEGWEISER DURCH DAS RÖMISCH-GERMANISCHE ZENTRAL-MUSEUM. Nr. 13: Numantia und seine Funde, von FRIEDRICH BEHN. Mainz, Selbstverlag des R-G. Zentral-Museums, 1931. pp. 48, figs. 1-33. 0.50 Rm.

This excellent guide to the Numantia collection in the above Museum gives a successful general sketch of the Iberian town and Roman siege-camps whence the collected material came. Both photographs and plans give the essential and typical without undue detail, and the text is beautifully clear. All students who cannot afford Schulten's monumental volumes, and all travellers who cannot carry them, must have this tiny book on their shelves or in their pockets : while the guardians of some of our own important Roman collections might emulate this example with advantage to the public and themselves.

I. A. RICHMOND.

EXCAVATIONS AT EUTRESIS IN BOEOTIA. By HETTY GOLDMAN. *Harvard University Press and Oxford University Press*, 1931. pp. 294 with 20 plates and 2 maps. 73s 6d.

Dr Goldman is to be congratulated on establishing by methodical excavation and prompt publication the Bronze Age culture-sequence for Central Greece that should have been worked out twenty years ago, had the excavations at Orchomenos or Hagia Marina been properly conducted or published. Now Eutresis provides for this neglected area a stratigraphically established succession of phases comparable to those long familiar in Crete: the Cyclades, the Peloponnese, Thessaly and even Macedonia.

The town lay on the road from the Gulf of Corinth to Thebes and, though unimportant in classical times, seems owing to this situation to have enjoyed a modest prosperity during the Bronze Age. The debris of prehistoric settlements has formed an accumulation 6.50 to 7 metres deep. Of this (apart from a few hut-foundations partly excavated in the subsoil and assigned to 'neolithic' times) 4 metres represent Early Helladic occupations, 2 Middle Helladic, and half a metre Late Helladic III (Late Mycenaean). It should be noticed that the authoress uses the term Middle Helladic in a cultural, not a chronological sense: Middle Helladic is extended to embrace the chronological phases termed in the Peloponnese Late Helladic I and II (the Early and Middle Mycenaean of Forsdyke), since the importations or imitations of Cretan pottery that distinguish these phases further south have not affected the Boeotian village. Making allowance for this discrepancy, it will be seen that the relative depths of the deposits is roughly proportionate to the absolute duration of the respective periods that might be expected on the analogy of the Cretan chronology: the two metres of Middle Helladic

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would represent the interval between 2000 and 1400 B.C.; the Early Helladic layers the preceding millennium. But since the Middle Helladic occupation was probably less intensive than the previous one, the above argument must not be strained. Dr Goldman herself, like Dr Frankfort, contends that Middle Helladic began with Middle Minoan I in Crete rather than with Middle Minoan II as Wace and Blegen have argued.

For the rest the publication, with its plans, excellent photographs and fine coloured plates (but scales are never given!) enriches our picture of the Aegean Bronze Age, the main outlines of which are already familiar. At the same time it opens up vistas of wider relations. The sherds from the neolithic hut-foundations can be exactly matched at Vinča (Dr Goldman like Dr Frankfort will write Vinča though the word is pronounced Vintsha) on the Serbian Danube, and so reinforce the idea of a cultural community spreading right across the Balkans which Dr Frankfort has so ably expounded (but if it be called Danubian that can only be in a quite different sense to that implied when speaking of say the Belgian Omalian) and favour a high dating for the Serbian site.

Then in the Early Helladic III level appear sherds decorated with triangles formed by the imprint of lengths of cord. These are justly termed 'corded ware' and naturally compared with the best known representatives of that group in Saxo-Thuringia. But Rosenberg's researches now suggest that these latter are but offshoots of a great family cradled in the Ukraine. The sherds from Eutresis, which are by no means unique in Greece, might then be brought into relations with battle-axes and other objects of South Russian type recently noted by Heurtley in Macedonia.

Finally Dr Goldman insists on the identity of the peculiar kiln-fired deoxidized grey ware termed Minyan in Greece with the grey ware of Anau in far Turkestan. And in the meantime comparable fabrics are turning up in Mesopotamia and even in India. So the thesis that this particular pottery was the invention of Central Greece—a thesis first expounded by the reviewer and subsequently espoused by Dr Frankfort—may have to be revised again.

V. GORDON CHILDE.

THE LONDON MERCURY



EDITED BY J. C. SQUIRE

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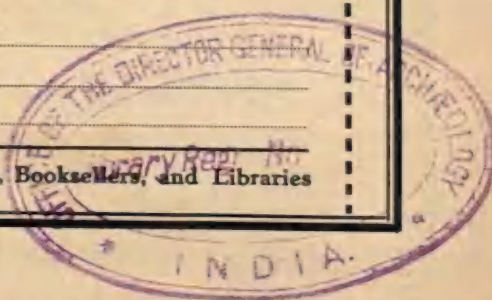
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A Quarterly Review of Archaeology

VOL. VI No. 24

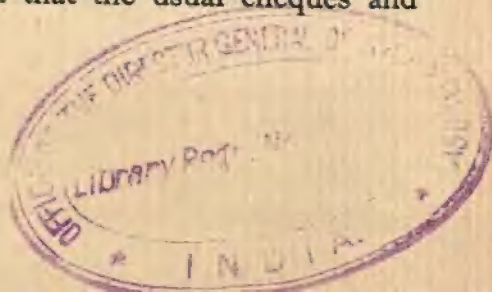
DECEMBER 1932

Editorial Notes

IT is sometimes difficult to decide what to say in these Notes ; and, always, to know how to say it. One must select a matter of general interest and importance, and one must express one's opinions without giving offence. The accepted rules eliminate certain easy targets of criticism, and if one does not criticize at all one is open to the accusation of log-rolling. We do not wish these Notes to fall to the level of some current literary journalism. But what is one to do if all avenues are closed, as at this time they usually are ? The digging season is over, and has been full of interesting results ; but one must not exploit other people's work before they may have been able to exploit it themselves. That is a pastime reserved for Fleet Street. The voice of the tempter whispers ' Why not a few words about ANTIQUITY ? It is December and they will be considering whether to continue their subscriptions '. But will this, after all, produce the desired effect ? Who can say ?



It is our usual custom at this time of year to draw attention to the completion of one more volume, and consequently that the subscription for the next becomes due. At the risk of reiteration we therefore remind our subscribers that the four numbers which are in contemplation for 1933 bring responsibilities, and that the usual cheques and



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notes will be welcomed. It does save much trouble (and even expense) if these payments are made early in the year—we do not, for various reasons, like having to send out ‘reminders’.



Last December our Notes dwelt on the economic situation which was then, and still is to some extent, affecting everyone. None the less we feel that in certain ways the effect can be allowed to carry one too far. We expected the withdrawal of subscriptions, and have received notices to that end, but in spite of this we congratulate ourselves that we have been able to produce ANTIQUITY for another year, and without curtailing its form or the number of illustrations. Further, we intend to do so in 1933. The diffusion of knowledge must go on in spite of depressions.



Some of our subscribers have, we know, been influenced by what we have said in former Notes, and have continued their support at some personal sacrifice because they felt this was not the time to withhold it. Others, to judge by results, have not been influenced—or perhaps (horrid thought) may have been influenced the other way!

It will be best to leave it at this, and trust to the goodwill and the same support of those who during the past six years have not failed us. The knowledge that we have them to rely on is one of the things which keeps us going.



There are times when we feel in need of some such ‘uplift’. Editing an illustrated book of over 500 closely-packed pages each year—and that is what it comes to—is not so easy a task as some may imagine. It would be less arduous if contributors sent articles and illustrations ready to go to press without further preparation. But this does not happen often. Some articles have to be translated into good English (and not all of these are of foreign origin), while for others illustrations have to be provided. Occasionally one for which we have asked comes prepared so carefully that little or no editing is needed, but in the majority of cases this is not our experience. It would be so easy, by taking a little thought, to lighten editorial work and with that in view we offer a few practical hints.

EDITORIAL NOTES

First, in the matter of photographic illustrations. Those in ANTIQUITY are printed from copper 'half-tone' blocks. Now what (for short) may be described as the 'grain' of the block is infinitely coarser than the 'definition' of the coarsest photographic contact-print. Consequently most of that fineness of detail is lost if a block is made from a contact-print—that is, a print of the same size as the original negative. Nowadays almost any negative will make a doubly enlarged print without any real loss of sharpness, and with a great gain of size and legibility. From this enlarged print a slightly reduced half-tone block can be made; the result is a picture that is still bigger than the original negative, but infinitely superior to the (smaller) one that would have been made from a contact-print.



The first rule, then, is always to supply enlarged, rather than contact, prints. It does not matter how large of course. A half-tone block 7 by 5 inches can be made from a print 34 by 28 inches, which itself may have been printed from a negative of 5 by 4 inches. It *does* matter, however, what sort of print is made. Here photographic skill comes in. Only a good photographer can make a good print; and only he, sometimes, after several failures: for every negative varies in density and requires therefore a different length of exposure. The ordinary commercial photographer is too often content with his first (unsuccessful) effort—usually a pale, half-baked affair, and the ordinary archaeologist is too often content to accept it. A good photographer can get wonderful results by enlarging from a poor negative, but no blockmaker in the world can make a good block from a thin or bad print.

If this simple matter were properly understood, there would be fewer eyesores in scientific publications.



The word 'eyesores' reminds us of certain illustrations—we will not specify them further. You may have reached the stage of getting the perfect enlarged print or the impeccable drawing or map, and still your labour and skill may be rendered useless, when published, by undue reduction. Illustrations are meant to be *seen*, and they are mere waste of money if they are illegible. Excessive reduction defeats the whole purpose of illustration, which is to throw light upon the subject, not to darken it. If it is a question of cost then surely one

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large picture is better than two small ones, if smallness carries with it a blurring of detail. These criticisms apply with particular force to the sketch-maps illustrating archaeological books and articles, to drawings of flints and to air-photographs. They can be avoided if the author is firm and the editor reasonable.

❧ ❧ ❧

The second rule comes logically before the first, but it lies beyond the control of the editor. It is—to take good photographs! How often have we suffered from photographs of some sunny eastern landscape reduced to desolation by the camera. Many a one suggests that, when it was taken, the elements had conspired together to produce simultaneously a dust-storm, rays of blinding white light from the ground, heavy rain, and a tilted horizon. Nowadays good photographs are quite easy to obtain with a very cheap camera. Films are easy to develop everywhere (at night if no dark room can be used). During a recent holiday we obtained over 400 negatives, nearly all good enough for reproduction *anywhere*, for a total cost, including the camera (but not of course the prints), of well under five pounds. Some are published in this number of ANTIQUITY.

❧ ❧ ❧

We write these Notes not merely for selfish reasons, but because there certainly exist many misconceptions on the subject, and because photography, and the publication of illustrations, is now admitted to be an essential part of the archaeologist's equipment. Every experienced excavator of course knows this: but the Notes are not written for him. There are many who do good archaeological work as a hobby, and it will often be vastly improved by attention to such details as these.

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We have placed in this number the usual form and envelope for the payment of subscriptions, to which we have referred in our second paragraph. We hope they will receive attention.

Ancient Cultivations

by E. CECIL CURWEN

THE ability to recognize the traces of ancient cultivation is essential to all students of earthworks and field-workers. The scope of our study is being widened, so that we are interested not merely in fortifications but in any kind of interference with the surface soil resulting from man's activities in the past. It would be purely an arbitrary distinction to be interested in a hill-fort and not in the surface traces of an agricultural village of the same date, and these same surface traces consist very largely in lynchets, or cultivation terraces, the presence of which is often the sole clue to the site of the settlement.

It is a matter of observation that agricultural processes at different periods may leave quite distinct and separate kinds of traces on the ground. This depends on the kind of plough used, and on different habits with regard to the shape, size, and outlay¹ of the fields. The one feature that is common to most ancient fields is the tendency to form terraces on sloping ground, due to the way in which the soil creeps downhill under the influence of ploughing and rain-wash, forming an accumulation along the lower edges of the plot.² In this way ancient fields are often perpetuated, especially on chalk hills, by scarps called *lynchets* which mark the limits of the plots, and often cover large areas with quite a chessboard pattern, or in other cases with step-like terraces. The accumulation of soil at the lower edges of the plots, distinguished by the term *positive lynchets*, naturally has its counterpart in the denudation of soil along the upper edges, leaving a scarp called a *negative lynchets*. A proper appreciation of this distinction is essential for elucidating the history of a series of lynchets, especially where there is reason to think that changes have occurred in the outlay of the plots, or where lynchets come into association with earthworks which may be earlier or later than the cultivations.

¹ The cacophonous and ungrammatical 'lay-out', an Americanism chiefly patronized by mushroom estate agents, is to be deprecated.

² The present paper is intended to supplement what was said in my paper in *ANTIQUITY*, 1927, I, 261-289, to which the reader is referred for much ground-work.

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Thus at Cissbury (Sussex)³ the southern rampart of the fort appeared to occupy the line of one of a series of lynchets, and it was desirable to determine whether the rampart had been thrown up on top of the crest of a pre-existing positive lynchet, or whether the soil forming the lynchet had accumulated in the angle behind a pre-existing rampart. A section cut through the rampart revealed that the latter alternative was the right one in the first instance, that is, that the rampart was made first; then came the ploughing; but finally a later rampart was constructed on top of the lynchet to heighten the first one. A study of the distribution of pottery fragments in these three deposits of soil—the first rampart, the lynchet, and the second rampart—led to the conclusion that the fort was constructed in the Iron Age, about 300 B.C., and that after its desertion about 50 B.C. the interior was ploughed up, both before and during the Roman period; while at some later date, probably at the time of the Saxon raids, the place was re-fortified by the additional rampart (fig. 1).

Again, on Thundersbarrow Hill (Sussex) there is a very extensive area covered by ancient fields, and in one part it looks very much as if a later series of lynchets had cut obliquely across the upper part of an earlier set, leaving the corners of some of the supposedly older fields peeping out as triangular patches from under the edge of the apparently later ones. Surface observation, aided and abetted by the air-photographs,⁴ might have made out an excellent case for a differentiation in date between these two sets of fields, but a few short sections dug into the lynchets revealed that the two series are separated by a negative lynchet, indicating that the 'earlier' set (which is situated lower on the hill-side) must have been under cultivation simultaneously with the 'later' set, and that therefore the oblique arrangement was merely a matter of topographical convenience, and does not indicate a difference of date (fig. 2).

The same hill provided yet another instance of the usefulness of recognizing the negative element in lynchet-formation. On the top of the hill is a small Iron Age fort, and a section dug through the western rampart failed to disclose any ditch at all, but only a scarp hollowed out of the solid chalk. The explanation is that the ground immediately outside the fort had been under the plough during the Roman period,

³ *Antiquaries Journal*, XI, 21-3.

⁴ The site is unfortunately marred by military trenches dug during the War, resulting in the complete destruction of one of the triangular patches and the obscuring of much of the rest.



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and an immense positive lynchet along the lower edge of the field has its counterpart in denudation of the upper edge of the field to a depth of some five feet, completely removing the ditch of the fort, and leaving the soil of the rampart perched on top of a bare chalk scarp which is in reality the negative lynchet. In ordinary parlance it may be said that the ditch had been 'ploughed away' (fig. 1).

As a result of surface observation, air-photography and excavation, it is now sufficiently established in this country that there is a field-system distinguished by small, squarish fields which is to be dated in round figures to the millennium 500 B.C. to A.D. 500, and which is best described as the Celtic field-system. In marked contrast to this is the English open-field system which had its origin in the obscurity of the Saxon conquest and lasted in some places down to comparatively recent times. There the principal distinguishing feature is that the individual plots are long and narrow, nominally a furlong (660 feet) in length, and a chain (66 feet) or less in breadth. On sloping ground these strips may be arranged parallel with the contours, in which case lynchets will form, making as it were a staircase up the hill; or they may be placed at right angles to the contours, each strip mounded up in the form of a ridge to facilitate drainage. Such ridges are a familiar feature in many counties, where parcels of them have often been enclosed by hedges to form large modern fields.

It will thus be seen that the Celtic, English and modern field-systems admit of ready distinction owing to the marked contrasts they present. But extremely little is known of the origin of the first two, or of the existence of any recognizable systems which could be attributed to the Bronze Age or earlier. Moreover, most of the work that has been done on this subject has been done in the south of England, and there are large gaps in our knowledge of ancient field-systems elsewhere. The sudden supersession of the Celtic by the English system in certain counties is a powerful argument for the completeness of the overthrow of the Celts by the Saxons in those parts, but in Kent, where peaceful penetration has been claimed by the historians, the Celtic field-system and its fate have not yet been studied. The Celtic type of field appears to persist to this day in those counties, such as Cornwall, to which the Saxons did not penetrate early.

With a view to stimulating research on this subject the Council of the Congress of Archaeological Societies is issuing (1932) a leaflet summarizing what is known about it, and giving guidance to field-workers and others as to the kind of thing to look for, and the features

PLATE I



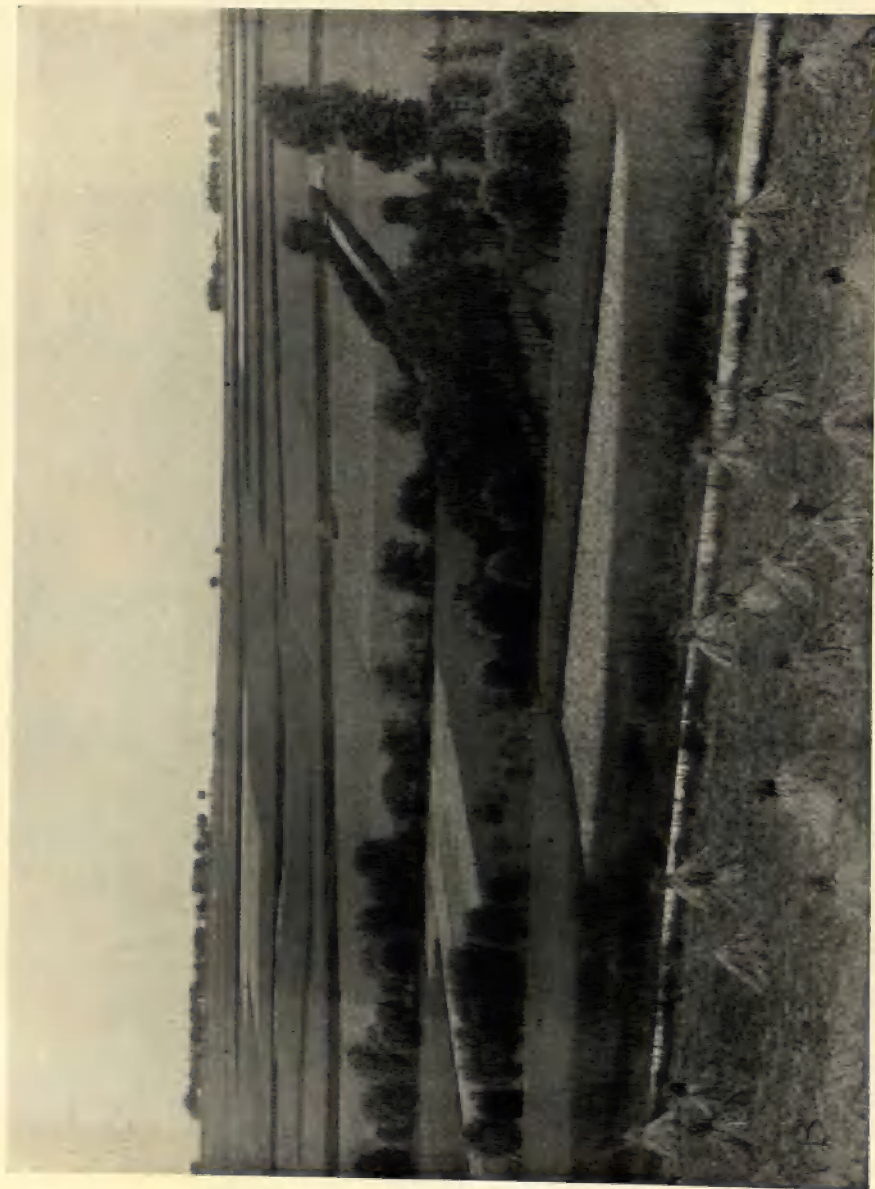
LYNCHET AT EU, SOMME, FRANCE
Ph. F. Burt

PLATE II



LYNCHETS AT EU, SOMME, FRANCE
Ph. F. Burt

PLATE III



LYNCHETS AT BELLIFONTAINE, SOMME, FRANCE
Ph. F. Burt

PLATE IV



STRIP-LYNCHETS IN PROCESS OF FORMATION, VRESSE, BELGIUM
Ph. E. C. Curwen

ANCIENT CULTIVATIONS

to note. It is hoped that wide use will be made of this, so that evidence may be gathered systematically all over the country. Individual lynchet-systems are, of course, dated principally by locating and dating the settlements with which they appear to be integrally associated.

Although no existing remains of fields of the Anglo-Saxon period are known, documentary evidence gives us no reason to think that the English field-system was derived from the Celtic in this country. On the contrary it appears to have been introduced *de novo* by the Anglo-Saxons from abroad, and there is some evidence that analogous systems with local variations may have been diffused through parts of western Europe by conquering Germanic tribes.⁵ But however this may be, the origin of both systems must clearly be sought on the Continent, and for this reason we are anxious to introduce the study to foreign archaeologists to whom for the most part it is as yet a *terra incognita*, though fields of Celtic type have been discovered in Holland by Dr van Giffen and described in *ANTIQUITY*.⁶ For this reason we welcome two recent papers which reveal the stirrings of interest in Denmark and France.

PREHISTORIC FIELDS IN JUTLAND⁷

Danish observers have long been familiar with traces of ancient deserted cultivations in their own country. Quite independently of similar researches in England, Professor Hatt has studied this subject in Denmark, making extensive observations, surveys and excavations. His results are embodied in this paper, in which he observes with satisfaction that his conclusions are in complete agreement with those of Crawford and other workers in England.*

The author recognizes five types of derelict cultivations in Denmark.

(1) Fairly modern fields, which have been tilled for two years and abandoned. This appears to have been a habit of some farmers towards the end of the eighteenth century

(2) The so-called 'Hochäcker', which correspond to the strip acres of the English open-field system, and are mounded up to form ridges, as is the case in many parts of England. This type is referred

⁵ E. C. Curwen, *Air Photography and Economic History* (Economic History Soc. Pamphlet no. 2).

⁶ *ANTIQUITY*, 1928, II, 85-7.

⁷ Gudmund Hatt, 'Prehistoric Fields in Jylland' (Jutland), published in English in *Acta Archaeologica*, II, pt. 2, 117-158. Illustrated with photographs and two plans. (Copenhagen, Levin and Munksgaard, 1931).

* Including the writer of this article and his father.—EDITOR.

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to medieval times, and the author correlates it with the introduction of the mould-board on the plough, whereby the sod is undercut and turned right over. This is a view to which we had already inclined.

(3) A type characterized by long, parallel rows of stones separating the field-strips, with additional heaps of stones often found lying round a large earth-fast stone, and considered to be the result of clearing the field. The length of the strips is, unfortunately, not stated, but the breadth is given as 11 metres (36 feet), and there is no ridging. In some of the associated heaps of stones a large quantity of potsherds was found, of pre-Roman character, from which fact the author assigns these fields to the earliest part of the Iron Age in Denmark. No strips of similar character and of such early date have yet been described in Britain, though perhaps the nearest approach may be found on Dartmoor, particularly in the neighbourhood of Kes Tor rock (ANTIQUITY, I, 277, 283, fig. 21).

(4) The fourth type is described in detail, and most closely approximates to our British cultivations of Celtic type. The fields are rectangular, varying in area from 1.75 to 0.05 acres, but most commonly falling between $\frac{1}{4}$ and $\frac{3}{4}$ acre. They are separated by banks, termed 'balks' by the author, 10 to 20 feet broad, and 4 to 12 inches high, consisting primarily of turf-mould collected from the surface of the ground when the fields were first laid out. The detailed interpretation of the sections of these balks is difficult to follow, owing to the use of technical terms such as 'podsol' and 'podsolation' which are not to be found in Murray.

Some fields of this kind on Byrsted Heath are associated with others, evidently contemporary and balk-enclosed, but which form long strips up to 1100 feet by 50 feet. These recall the strips in type 3, the only difference being, it seems to us, that the field-boundaries consist of collected stones in the one case, and of collected turves in the other; and it does not seem to us that such a distinction has any practical value, possibly depending only upon the nature of the ground. Balks like those here described must be rare or absent in Britain, where the fields are usually demarcated by lynchets. At Park Brow (Sussex) balks consisting of gathered flints occur—the only instance of which we are aware. Lynchets, about 3 feet high, also occur among the Danish fields. We cannot help feeling that such long strips as are here described as being associated with rectangular fields, must have been transversely divided into squarish plots, not always marked by

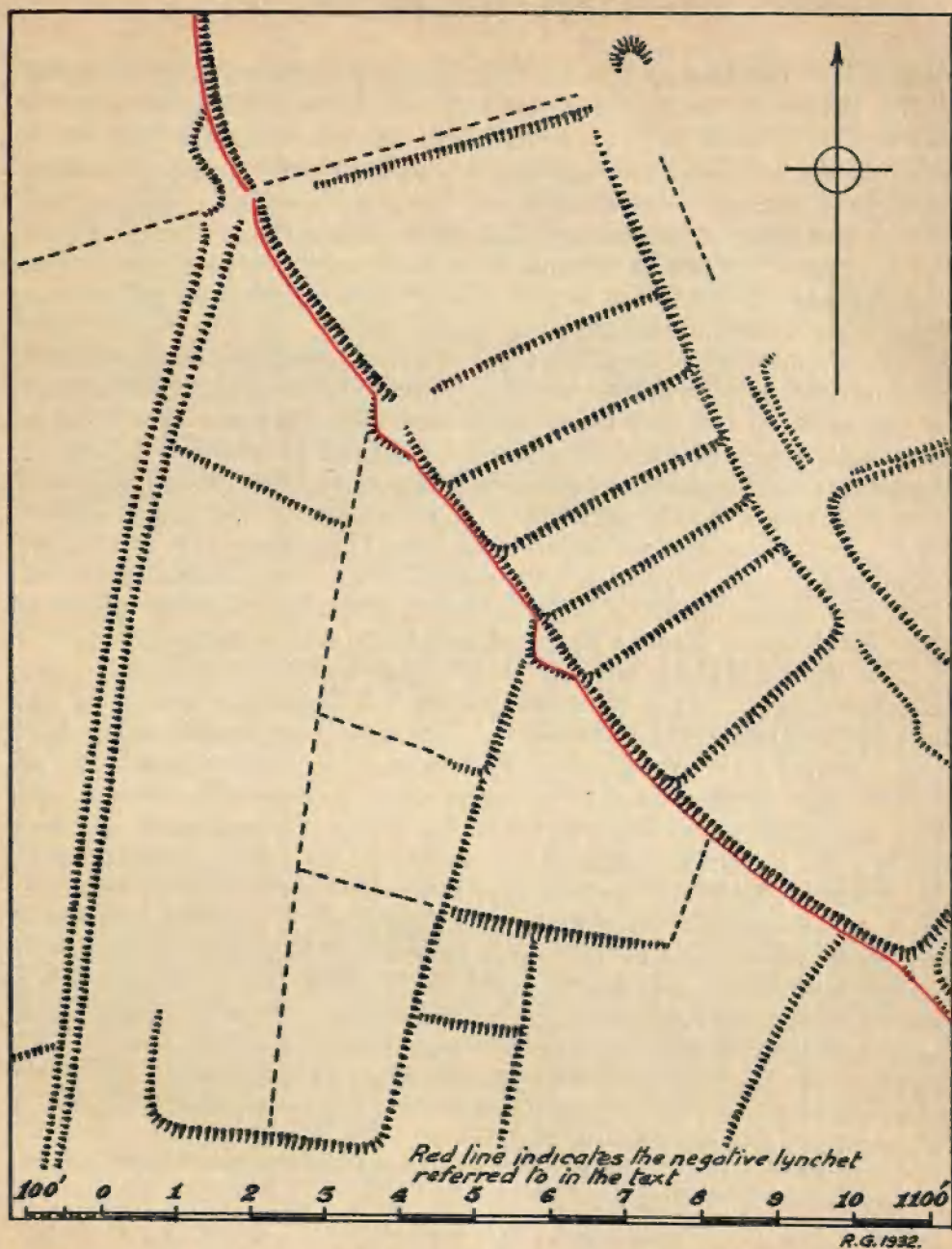


FIG. 2. LYNCHETS ON THUNDERSBARROW HILL, SUSSEX, ILLUSTRATING THE DIAGNOSTIC VALUE OF NEGATIVE LYNCHETS

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permanent boundaries. Something analogous may be seen in British Celtic fields, as for instance, on Charlton Down, Wilts (ANTIQUITY, I, 276, fig. 4), and in the strip-cultivations still in use in parts of the Isle of Skye (*ibid.* 277, figs. 12, 13, and p. 278, footnote). These latter may measure 1200 feet by 120 feet, and are transversely divided by non-permanent boundaries into four or five plots which subserve the purposes of rotation of crops for a single owner. In any case there is a marked tendency for British Celtic fields to be arranged in long rows, collectively resembling the Danish strips.

As illustrating fields of type 4, three groups are described in detail, the description being accompanied by excellent plans from surveys made by the author. In only one instance—that of Byrsted Heath—was an associated field-way noted (this term is preferable to 'alley'). At this site the author collected several samples of soil both from the old fields and from the virgin heath, and found confirmation of the observation of Dr O. Arrhenius that soil which has been cultivated in former times contains a greater proportion of phosphoric acid than uncultivated soil. This hint is one which is perhaps worth following up in our efforts to trace Neolithic or Bronze Age cultivations on our western moors.

At Albæk Heath another rather new feature of interest arises. The fields here (type 4) are covered by hundreds of what the author calls by the Danish term 'Rodvæltet' for lack of an English equivalent. Each consists of a mound some 20 feet long by 10 feet broad, and 18 inches high, accompanied by a corresponding depression on one side. This is the characteristic mark left by a tree that fell long ago in a storm; the root of the falling tree has turned up a mass of soil forming the mound which may persist long after the tree itself has vanished, while the depression represents the place from which the roots have been torn when the tree fell. We have long been familiar with very similar features in the beech-woods on the Downs in West Sussex, but have hitherto regarded them as old saw-pits dug by wood-cutters to enable them to get underneath a felled tree-trunk for the purpose of sawing it up more easily. The Danish Rodvæltet represent, therefore, a forest which grew up after the desertion of the cultivations, and which in turn was succeeded by the heath.*

* Precisely similar pit-and-bank groups are found on the limestone uplands of the Cotswolds, and were formerly mistaken for pit-dwellings. Good examples may be seen between Avening and Nailsworth and on Minchinhampton and Selsley Commons. Dr Hatt's explanation was put forward by me quite independently many years ago, but I do not think I ever published it. It is satisfactory to find that it is supported by the Danish evidence.—O.G.S.C.

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The date of these fields of type 4 is provided by the discovery in some cases of associated settlements, which are attributable to the pre-Roman Iron Age, several fine specimens of pottery vessels being illustrated by photographs. Fragments of similar pottery were occasionally found under heaps of gathered stones in the corners of the fields. This dating is, of course, in complete agreement with that of the corresponding type of cultivation in England.

In addition to habitation-sites, some burials of the same period have been found, either in or on the balks, consisting of small barrows containing cremations and pottery. Similar small barrows have occasionally been observed on the crests of lynchets in Sussex, as for example on Windover Hill (ANTIQUITY, I, plate II, f.p. 272), and on Kithurst Hill. In this category should also be placed, perhaps, the four first century urn-burials recently discovered in the body of a very large lynchet near Lewes. We may yet discover that it was a habit among the Celts to bury on the edges of their fields, in which case one is tempted to surmise whether they may have done it from a superstitious motive with a view to increasing the fertility of the ground.

With regard to the purpose of the balks or field-banks, the author suggests that they may have been intended to conserve surface water, and that, if so, this may suggest an origin for the system in a drier climate. Such a theory is, however, too poor in evidence to be worth much, and is not really strengthened by the further suggestion that the simple type of plough which must have been used is still seen in Mediterranean regions. After all, our very knowledge of agriculture came ultimately from that quarter, and so did every successive improvement in methods. The author is, however, undoubtedly quite right in attributing these small flat fields to a simple type of plough without a mould-board. This he terms an *ard*, but unfortunately does not describe it, though he rightly contrasts it with the larger wheel-plough with mould-board which was responsible for the high-ridged strips called by the Germans 'Hochäcker'. He suggests that the introduction of the larger plough led to the desertion of these earlier fields and a migration of the population to heavier and more fertile soils. In England this is just what seems to have happened, but it was done by invading Teutons. Professor Hatt does not, however, venture into the realms of ethnology in this connexion.

He regards these groups of fields as representing an old form of open-field system. In many cases a field has evidently been divided

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into two or more almost equal parts by transverse balks which are lower and narrower than the others, and he suggests that this feature may indicate a dividing of fields between inheritors.

A list is given of sixty-nine sites in Jutland where fields of type 4 are to be found.

(5) The last type in the author's description of ancient cultivations is peculiar in the entire absence of any kind of field-boundary or of ridges or furrows. The only feature suggesting former cultivation is the fact that the stones are gathered into irregularly placed heaps over an area, it may be, of 50 to 70 acres. Under one of these heaps the author found neolithic potsherds which leads him to attribute the whole system to that period, though he adds a proviso to the effect that under another heap he found Iron Age sherds. He hesitates, therefore, to assign all the 'fields' of type 5 to a date earlier than the Iron Age, but regards them as more primitive. This is the weakest part of the whole paper. That these cultivations (assuming them to be such) should be of neolithic date is quite possible, but we should welcome more definite evidence. They would certainly fit in with the view that neolithic man tilled the ground only with antler picks or similar tools which do not make furrows.

Professor Hatt's paper is altogether admirable, and should be a pattern to workers in other countries.

LYNCHETS IN FRANCE

The second paper is by a French geographer, M. Léon Aufrère,^a who approaches the subject of lynchets from the standpoint of a geographer rather than that of an archaeologist. The subject has not hitherto been properly studied in France, and the author has not been acquainted with the results of similar research in England. It is all the more to his credit that he has recognized that lynchets are the results of human activity in agriculture, though he finds it difficult to persuade his countrymen of this, and is at much pains to do so in this paper. This is all the more surprising seeing that many French lynchets are still under cultivation. His arguments are excellent, if somewhat superfluous to English ears. Nevertheless his paper is full of interesting and useful points.

^a Léon Aufrère, 'Les Rideaux : étude topographique', published in *Annales de Géographie* (Paris, 1929), xxxviii, 529-560, illustrated.

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He reviews and rightly rejects the quite impossible theories that have hitherto held the field in regard to the causation of lynchets—those of water action, the slipping of clay slopes, raised beaches, and earthquakes (!)—and substitutes that of agriculture, though he does not claim to be the first to do so. It seems strange to us that this should not have been recognized before, but the fact is that for all their magnificent achievements in other branches of archaeology the French are not students of earthworks, and have not developed that 'field-sense' which for the field-worker corresponds to the motorist's 'road-sense' or the physician's 'clinical acumen'. It is in the possession of such a guiding instinct based upon experience and common sense that this author scores over some of his *confrères*.

The French term *rideau*, applied equally to lynchets in the north and to vineyard terraces in the south, signifies literally a fold in a curtain, to which the scarps of lynchets are likened. This is a legal rather than a popular term, and Godefroy's dictionary defines it thus:—'*Un rideau est une langue de terrain escarpé et en pente qui sépare deux héritages voisins*', i.e., a long, narrow scarp which separates two contiguous plots belonging to different owners. Another term in common use among peasants in the north of France is *royon*, *réyon*, *riyon*, or *rouyen*, the root of which is *roye* or *raye*, i.e., the furrow made by the plough. *Enroyer* and *déroyer* mean respectively to make the first and the last furrow in a field. It seems to us likely that this is the origin not only of the term 'rhine' which is applied to the drainage ditches in the flat parts of Somerset, but also of the word 'rein' which, we understand, is synonymous with 'lynchet' in Yorkshire.

Besides these there are an immense number of local terms applied to lynchets, most of which betray the agricultural, or at least human, origin of these banks. *Fossé* and *douve* signify primarily a ditch, and are thus comparable to *royon*. *Rive* means edge or margin, while *fraite* (from Lat. *fracta*) signified first a gap in the continuity of a lynchet designed to let carts into or out of a field, and then any field-way, especially those that run below a lynchet. *Croc* is a term applied equally to lynchets or sand-dunes, and seems likely to be the Celtic *cruc*, a ridge or mound (though the author attempts to derive it otherwise, and less convincingly).

The author then gives a minute and accurate description of the usual profile presented by lynchets, noting especially the raised crest and the slight hollowing of the ground at the foot of the scarp, and showing how these features result from ploughing, and especially from

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turning the sods over downhill, year after year. Thus the upper half of the slope of the lynchet consists of the accumulation of soil belonging to the field above, while the lower half is due to the denudation of soil at the top of the lower field. If the two fields belong to different owners, the slope of the lynchet must therefore be shared between them, and this recognition of the positive and negative elements⁹ in lynchet-formation is revealed in local customs. The commonest arrangement is that the slope of the lynchet belongs to the owner of the lower field, except such part as may be covered by the legs of the owner of the upper field when he is sitting on the crest of the lynchet with his legs hanging down. This amount is therefore called *la jambe pendante*. The two neighbours often try to widen their fields at the expense of the stability of the lynchet-face, with disastrous results to both. Local custom does not allow the lower owner to destroy the lynchet, either by undercutting it with a pick, or by cultivating it, for once the protecting mantle of turf is gone nothing can prevent it from crumbling. The grass is, however, burnt to prevent it from seeding among the corn. A case is on record where a hedge planted on the crest of a lynchet by the owner of the upper field gradually slipped down the face of the lynchet till it came below the *jambe pendante*, when it was promptly claimed by the lower owner, and a lawsuit followed.

'Double lynchets' occur in France, as in England—that is, the positive and negative elements are separated in such cases by a narrow strip of original ground-surface. In this country we have long recognized that the intervening strip represents an old field-way giving access to the fields: M. Aufrère, however, does not mention this, but gives us the French terms *riez* and *larri* for this feature, endorsing the suggested derivation of the latter from Lat. *latericus*, 'lateral'. In this case *via laterica* would mean a road along the flank of a lynchet—a neater term than our clumsy 'double lynchet road'.

The relation between the height of a lynchet and the steepness of the hill-side is discussed, and one gathers that though the actual height of the lynchet may vary with the steepness of the slope, the volume of soil displaced does not vary much—the number of years of cultivation being, of course, constant. The volume of soil displaced varies according to the method of ploughing. The easiest way was always to turn the sods downhill, but this soon resulted in the sterility of the upper part of the field; in recent years sods are turned uphill

⁹ The author uses respectively the terms *remblai* and *entaille* for this.

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one year and downhill the next, in order to minimize lynchet-formation. In some parts, as in parts of England, soil is actually carried uphill in carts for this purpose.

A study of the sections revealed when lynchets are cut through affords the author an opportunity for recording many sage observations bearing on the nature of their causation. These principally go to demonstrate the impossibility of the theories of solifluxion or natural erosion to explain the facts. Similarly the raised beach and river terrace theories are disposed of by a detailed consideration of the longitudinal elevation of lynchets.

With the paper by M. Aufrère there are three plans¹⁰ and two good photographs of lynchet groups still under cultivation. These plans are discussed in considerable detail, but the gist of the matter is that the particular groups concerned were originally laid out on the *journal* system, the local *journal* corresponding with the English acre, both in area (42.21 acres) and in length of furrow (660 feet). (The term *journal* signifies literally a day's work in ploughing). Two successive periods of lynchet formation are recognized: in the first the ground was laid out in parcels generally containing an integral number of *journaux*, up to six, the length, corresponding to the furrow, varying from 180 to 260 metres (nominally 201 m.), and the breadth roughly half that amount. The secondary lynchets divide these parcels into strips, containing, as a rule, $1\frac{1}{2}$ or 2 *journaux*, or less. Further subdivision, probably more recent, has cut these strips up in two ways: some are split longitudinally into extremely narrow strips in which there is scarcely room to turn the plough; others are divided transversely into four or six squarish plots which closely correspond in size to the old Gaulish *arepennis*. This mixture of strips and squares is most peculiar, and further work, particularly study of documentary evidence, needs to be undertaken, in order to understand its historical significance. One point, however, is brought out: the *journal* was not introduced till after the Carolingian epoch when the *bonnier* was in use; if this is so, these lynchets must be regarded as medieval in origin. We need now to know what the Gallo-Roman system was like, and whether there is any connexion or continuity between it and later systems.

¹⁰ The scale given for fig. 5 is evidently an error due to the plan having been reduced by the editor more than the author intended. Instead of '1 : 5000' the scale seems to be approximately 1 : 6000. A drawn scale would have obviated this error, and have been easier to use.

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I am indebted to Mr J. Pelham Maitland for sending me the photographs of lynchets at Bellifontaine and Eu (Somme) illustrated in plates I-III.

LYNCHETS IN THE SCOTTISH BORDER DISTRICTS

In the south of Scotland and in Northumberland occur isolated groups of strip-lynchets, formerly called 'daisses' (*i.e.*, daïses, or shelves), producing very remarkable staircase effects on steep hill-sides. These have been the subject of two recent papers by Mr Robert Eckford, of H.M. Geological Survey, Edinburgh, and Mr W. W. T. Hannah, respectively.¹¹ Mr Eckford cites two groups in Lanarkshire, five in Peeblesshire, one in Midlothian, two in Roxburghshire, two in Berwickshire, and three in Northumberland. To the last-mentioned should be added the group at Housesteads recently described by Mr Percy Hedley.¹²

Each group consists of a number of long, narrow terraces, averaging 250 yards* in length and varying from 6 to 100 feet in width in different sites. The width varies inversely with the steepness of the slope, but commonly corresponds to a half, one or two rods, approximately. The faces of the terraces may be as much as 8 feet high, and some have been built up (as least partly) of stone. The terraces are generally co-terminous; that is, they all end together, sometimes at right angles to a butte or furrow which runs up the slope. They are not as a rule strictly level, but present a very uniform appearance, and excavation has shown that they are composed of fertile soil, sometimes containing charcoal. Scale-drawn sections across them would have been welcome, so that one might form an opinion as to whether the terraces had been artificially constructed or are merely the result of ploughing. They are found at various altitudes up to 1150 feet, the Dunsyre group being the highest; that this is not too high for cultivation purposes is shown by evidence of cultivation elsewhere in the same district at 1000 feet and even more. All the terrace-groups face either west or south, and are thus sheltered from the northeast winds while collecting the maximum sunshine. It seems to us almost as if these early cultivators

¹¹ R. Eckford, 'On Certain Terrace Formations in the South of Scotland and on the English Side of the Border', *Proc. Soc. Ant. Scot.*, LXII, 107-20; W.W. T. Hannah, 'The Romano Terraces: their Origin and Purpose', *op. cit.*, LXV, 388-98.

¹² ANTIQUITY, v, 351-4.

* According to Seeböhm (*Customary Acres*, 109-10), the furrow of the Scottish customary acre was 235 yds. (227 m.), while the Northumbrian was 280 yds. (256 m.).

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realized that the greater obliquity of the sun's rays in a northern latitude could be to some extent compensated for by cultivating steep south-facing slopes. This certainly seems to be a possible cause for their otherwise peculiar situation, for, as pointed out by Mr Eckford, in early days the valleys were too wet and wooded for agriculture to be successful. Very similar terraces may be seen actually being formed by ploughing at the present day on the Continent, as, for instance, at Vresse in Belgium (plate iv*).

Mr Eckford says—' All these terrace groups are in close proximity to an old fort or fortalice. At most of the forts in Peeblesshire no trace of terracing is found ; yet there are no terraces without a fort or ancient tower, or suggestion of such in its proximity '. Unfortunately the term ' fort ' is ambiguous ; does he mean it in the prehistoric sense, or in that of a medieval peel-tower ? Also, such a tower should be within 100 yards or so of the terraces if any connexion is to be argued. He cites Professor P. Hume Brown as saying—' Long after the time of Mary [Queen of Scots], an Englishman thus refers to the custom [of cultivating the hill-slopes] : " 'Tis almost incredible how much of the mountain they plough where the declensions—I had almost said precipices—are such that to our thinking it puts them to greater difficulty and charge to carry out their work than they need be at in draining the valleys " '.

Much of Mr Eckford's paper (echoed by Mr. Hannah) is concerned with proving the human, and probably the agricultural, origin of these terraces. His arguments are sound and, we think, quite conclusive, and by this time the matter should be considered as settled in Scotland as it is in England. Complete agreement has hitherto been delayed by the late Professor Gregory's unfortunate and untenable theory of solifluxion which is very properly demolished by Mr Eckford. Let us now cease to argue these fundamental points, and proceed with the superstructure—that of determining their date and the people who are responsible for having made them.

To this end it will be necessary primarily to collect the factors which are common to all groups, particularly in regard to evidence of occupation in the immediate neighbourhood ; Mr Eckford's observation *re* the forts and fortalices is a case in point, but something in the nature

* The blurring of this photograph is due to its having been taken from a moving charabanc—the only chance of getting it at all.

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of vanished manors (or their northern equivalents) should also be sought for.

Without desiring to prejudge the issue, it may be worth saying that these terraces are extremely unlikely to antedate the Anglian conquest. We know, both from Grassington¹³ in Yorkshire and Housesteads¹⁴ in Northumberland, what Romano-British cultivations in the north of England were like. At Housesteads the Romano-British lynchets were overlaid by a series of strip-lynchets of the class we are considering, thus giving the *terminus a quo* for the latter. Also, it is probably correct to say that these strip-lynchets are confined to the area of Anglian conquest in the north, though similar strip-lynchets are found in southern England, especially in Wessex. This does not necessarily imply that they are Anglian in date, for they may well be much later, though Dr Raistrick and Miss Chapman attribute the very similar Yorkshire examples to the seventh-ninth centuries.¹⁵ Mr Eckford says that in the Purves Hill group (Peeblesshire) ash trees grow on the slopes of the lynchets, while the flats of the terraces are bare. This is a very strong argument for a comparatively recent date for that series of cultivations. In the present writer's experience bushes, hedges and trees are by no means stable landmarks when considered over long periods of time, for the old trees die off and the seedlings spread. If, therefore, anything in the nature of an overgrown hedge is found *in situ*, its age cannot well exceed one or at most two generations of that kind of tree. In Sussex, bushes are entirely independent of Romano-British field-boundaries.

Large areas of the border hills are covered with the ridges of former cultivation, which invariably run up and down hillsides, not along them. Whatever may be the date of these ridges (Napoleonic War?) the strip-lynchets are clearly something quite distinct and represent a different habit of ploughing. Moreover they only occur sporadically in a few sites. Any theory as to their date must take these facts into account.

RIDGES IN THE WESTERN ISLANDS

During a holiday in some of the Western Isles of Scotland in 1929, the writer observed that in many places every available square yard of certain portions of rocky and hilly islands was covered with ridges and

¹³ ANTIQUITY, II, 168-72; III, 165-81.

¹⁴ Ibid. V, 351-4.

¹⁵ Ibid. III, 181.

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furrows indicating former cultivation. This was notably the case on the eastern part of Kerrera, opposite Oban, and over a very large part of Iona, including the slopes of the rocky hill, Dun-I. Even on the lonely and uninhabited Lunga, one of the Treshnish Isles, walled round by mighty cliffs and now the home of innumerable seabirds, a small square field was found, covered with these same ridges.

Each ridge is on the average about 6 to 10 feet wide, but the length varies in accordance with the space available between rocks. Those on Lunga are artificially limited to 135 feet in length. On the flat parts of Iona they are quite long (unfortunately we did not think of measuring them at the time), but up on Dun-I we found a group of three parallel ridges only 18 feet long, with outcrops of rock at each end, showing that they could never have been formed by ploughing, but by some kind of digging. Enquiries among the inhabitants as to their origin and history met with little success, but one boatman at Iona attributed them to the use of the caschrom, or Highland foot-plough, described and illustrated in a former article in *ANTIQUITY*.¹⁶ In this he was probably correct, for the ridges illustrate very clearly the description of the agriculture of the Western Highlands and Islands given in the *Statistical Account* in the eighteenth century, in which the caschrom is stated to have been in universal use.¹⁷ The ridging, which was done for the purpose of draining the ground, must have been accomplished by turning the sods, year after year, away from the furrows and towards the centre of the ridges, the effect of which would be very literally cumulative. The caschrom would be quite capable of doing this, for though it is only an improved digging-stick, it nevertheless makes a furrow and turns the sod over to the crofter's left as he works backwards, up the slope of the hill.

The number of these ridges indicate a considerable population in districts now sparsely inhabited. Probably they are not much, if at all, later in date than the end of the eighteenth century, though the system may go back much earlier.

The above will serve as illustrations of the abundant field-work that is waiting to be done both in Britain and in neighbouring countries before we can hope to obtain a comprehensive view of the development of field-systems—both geographically and chronologically.

¹⁶ I, 261, 269.

¹⁷ *Stat. Account of Scotland*, vi, 288-9 (1793).

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ADDENDUM

Since writing this article I have read the informative paper by Mr G. W. B. Huntingford on 'Ancient Agriculture' printed in the June number of ANTIQUITY, and noted his helpful criticisms of some remarks of mine on Prehistoric Agriculture in volume 1 (1927), 261-89.

Most of his criticisms I am prepared to accept, particularly that which concerns the ability of Roman ploughs to turn over the soil. It does not, however, necessarily follow that Romano-British agriculture was abreast of that in Italy. Evidence of *ridging* in Romano-British fields is extremely rare: Mr Crawford has shown me air-photographs of two such sites, one near Purbeck, and one on Charlton Down, Wilts. The vast majority were definitely not ridged. Most Celtic lynchets may have had their formation accelerated by turning the sods downhill, but they can scarcely have been formed intentionally, as they produce no advantage, but rather the contrary. The slope of the hill is not materially affected, while the area of the plot is slightly reduced thereby, and a strip along the upper edge is rendered relatively infertile by the negative lynchet formation. The suggested origin of square and strip acres is based rather on primitive tendencies than on developed practice, aided by the notorious conservatism of agriculture. I do not believe that cross-ploughing was practised in Britain, but it may have been by those who first invented the square acre, wherever that may have taken place. Similarly those who invented the strip acre broke away from the square tradition for definite reasons, chiefly consisting of the possession of larger and more effective ploughs and larger teams of oxen which made possible longer furrows and therefore narrower plots, and less time wasted in frequent turning of the plough.

Mr Huntingford's account of Nandi agriculture coincides closely with my own conception of neolithic agriculture in this country.

Bee-hive Dwellings of Apulia

by LANCELOT G. BARK

ANY traveller in South Italy who finds himself in that tract of bleak and desolate country which is intersected by the Bari-Brindisi railroad, will not fail to observe many odd-looking structures in the fields. At a distance they appear to be quaint little conical-shaped huts, some 8 or 9 feet in height, fashioned after the manner of round bee-hives, and composed of flat stones, loosely piled together. They are used, so the natives will explain, as shelters for the cattle, as barns, or as store-houses for the fishermen's nets.

But further enquiry will elicit some stranger information about them—to the effect that these structures date from time immemorial, and that they are called 'trulli', although nobody seems to know the reason why, or what the term really means. The traveller will also be told that if he is enterprising enough to make the adventure into the remoter parts of Apulia, he may see a whole city of 'trulli', a city of some 12,000 souls, who live in what must surely be regarded as among the strangest human habitations on earth.

The idea attracted me: I would see this city, although the way was said to be toilsome and the scenery unattractive. For the low range of hills which form the southeast termination of the Apennine range must first be traversed before one gains the high-road to Alberobello—such is the name of the city. But that range having been crossed a new world comes into view. It is a veritable world of stone. On every hand, stone in embarrassing profusion. Piles of stone along the road sides; piles in the corners of the fields; the walls between the fields built to a thickness which would serve for the walls of a city; the very fields themselves streaky with white ridges of limestone as far as the eye can reach. Stone, and stone of a particularly fine quality, everywhere.

Nature has been not only prodigal in the quantity, but thoughtful as to how her gift should be bestowed. For the stone is generally in the form of small flat slabs near the surface of the soil, easy to retrieve, and easy to manipulate.

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Away in the distance another trullo is visible, and yet another. They seem to be larger than those seen from the railway, though similar in pattern. Smoke is ascending from a tiny chimney in the roof of one of them. No stables or barns are these. We have arrived in 'trulli land'. These trulli are human habitations.

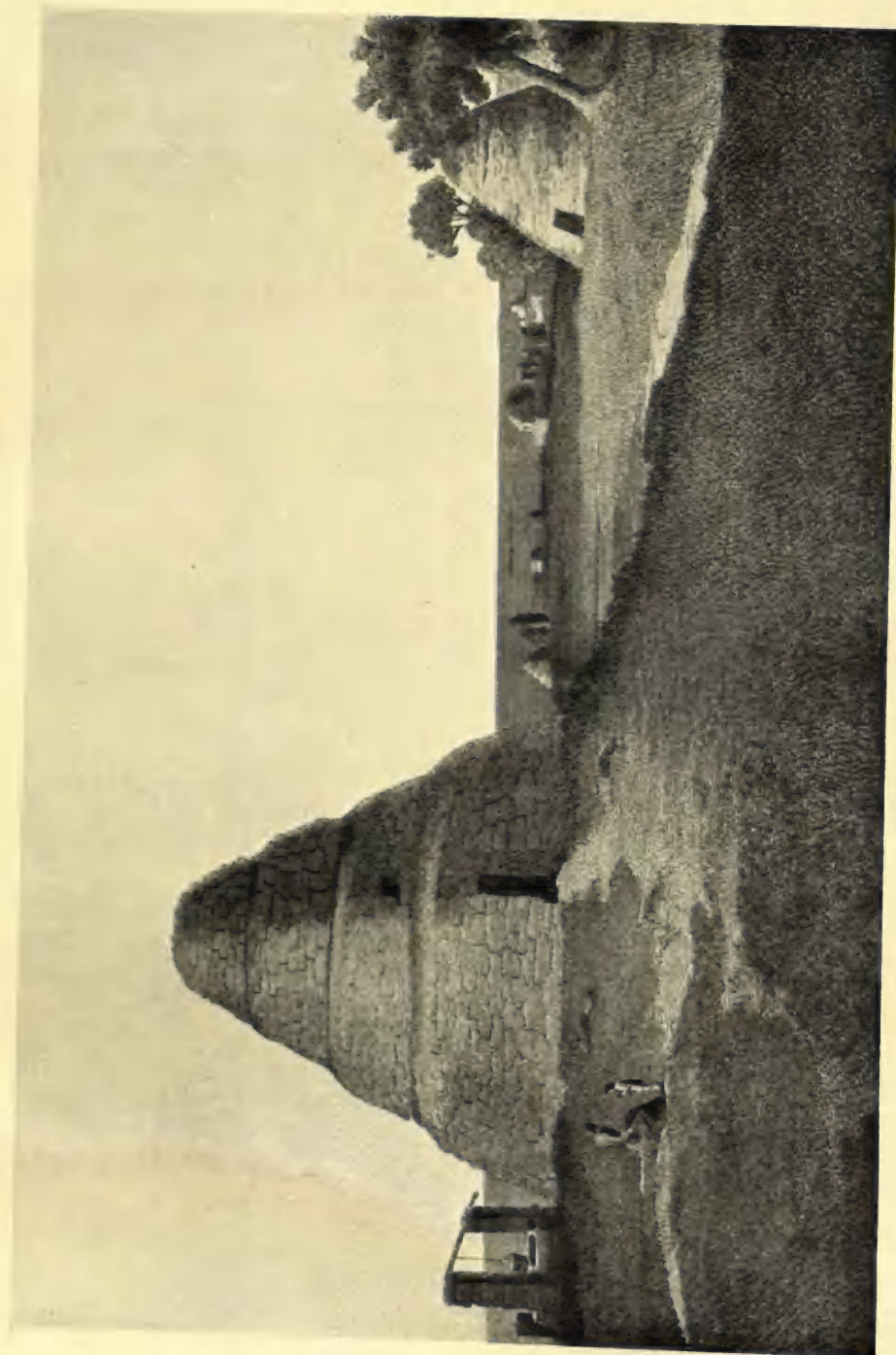
Another few miles and Alberobello is reached. It is a city which produces an impression without parallel. It is unreal and wildly fantastic. For Alberobello consists of street after street of trulli. It is a veritable forest of stone bee-hives, or perhaps hay-stacks would be a better simile, as many of them are from 15 to 20 feet in height.

With the aid of a friendly old peasant I was enabled to make an examination of several specimens. My peasant was a native, although he had lived many years in what he called 'Noo Yark', and had now returned to the scene of his birth, and appeared to be regarded as a sort of chieftain of the tribe. His own house was a typical example. About 20 feet in height, the walls built of flat slabs of limestone to a thickness of 7 feet, stacked one on top of the other, with neither mortar nor cement. From the top of the door upwards the roof contracts and gradually assumes a conical shape up to its summit, the topmost peak being crowned with a rude lump of cement by way of ornament. Inside, the dwelling was divided into two unequal sections, thus forming a living room and a bed room, the walls being covered with plaster to keep draughts out. A solitary fireplace stood in the centre: there was no flue for the smoke. The house possessed but one door and no windows.

It will be observed from this brief description that architectural laws are frankly set at defiance, and I was unable to perceive how the structure was kept together in the absence of mortar or cement, and apparently with nothing by way of a keystone in the roof. My friend explained, and showed me on his own roof how the stones were laid in circular fashion as the roof ascends, everyone wedging its neighbour. Thus each stone is virtually a keystone.

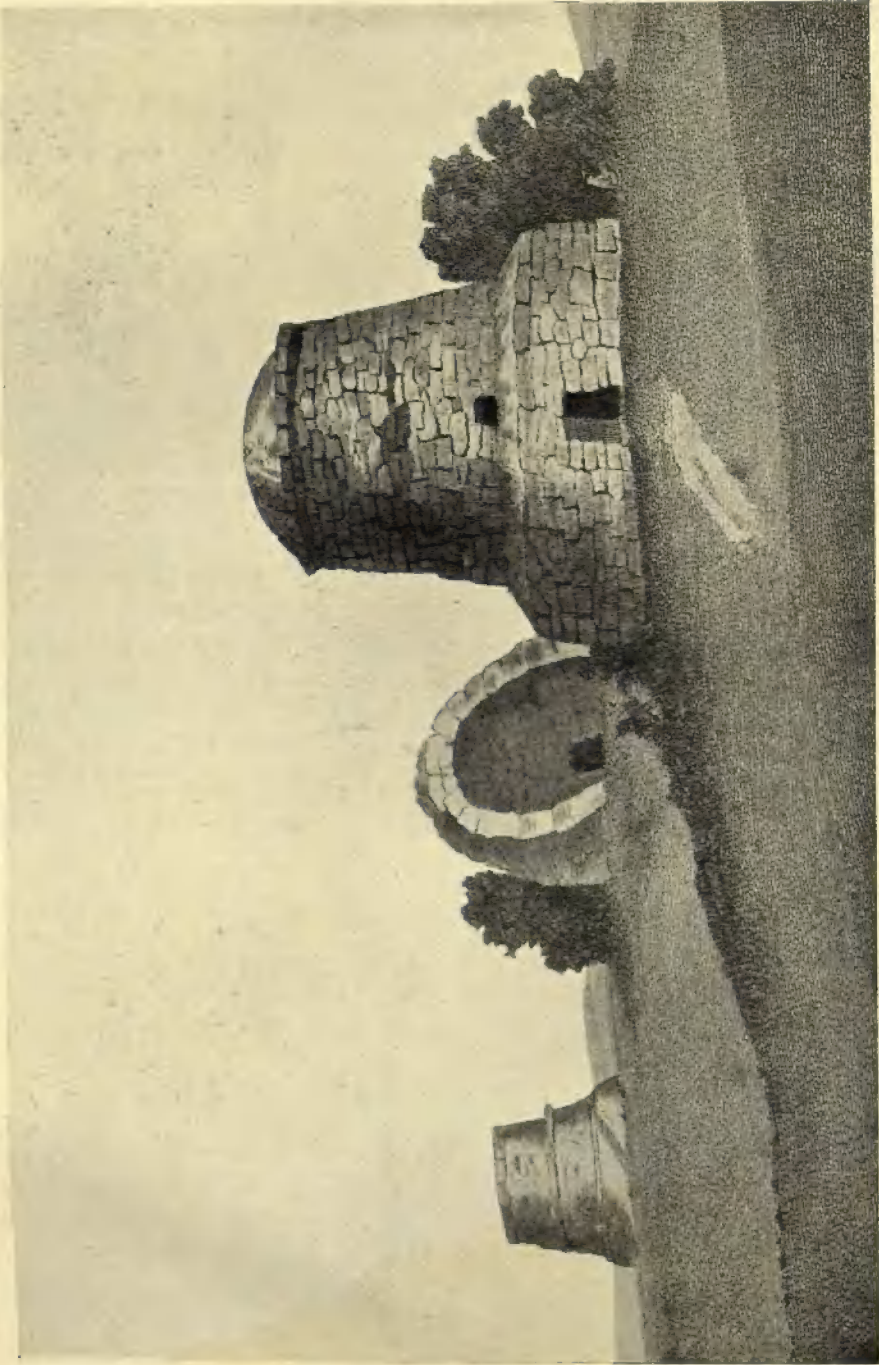
As we wandered down the 'main street', followed by a wondering crowd, for a visitor is a rare event in Alberobello, the old man answered my many questions. He assured me that these strange dwellings, dating as they do from time immemorial—for they seem to last for ever—were not constructed thus for purposes of defence, as some writers have supposed. They are what they are simply because their builders found it necessary to rid their land of stone before they could cultivate the soil. They built in a way which would use as much material as

PLATE I



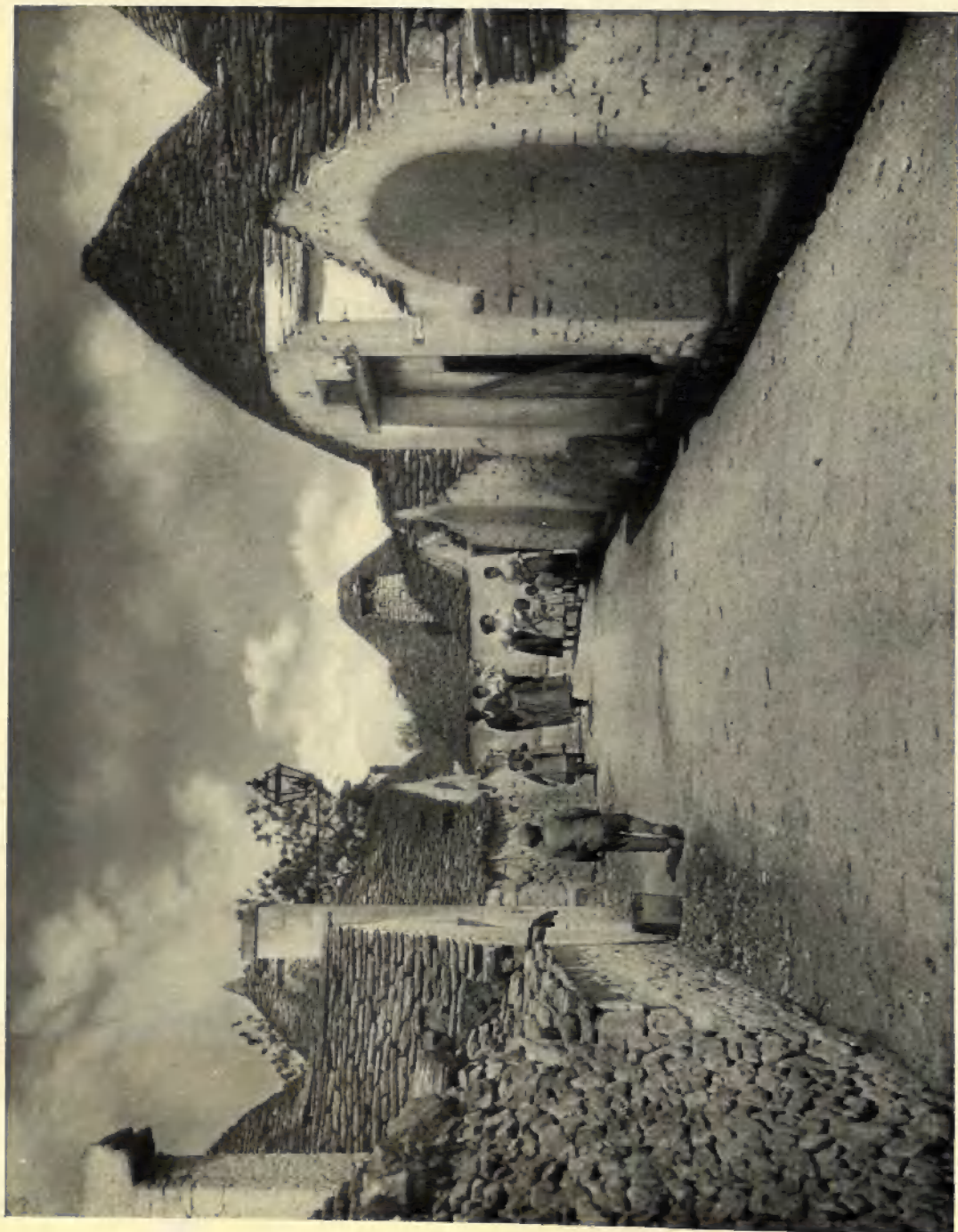
'WINTER HUTS ON THE SHORE OF THE ADRIATIC.'
From an old book of travel, probably 18th century

PLATE II



'WINTER HUTS ON THE SHORE OF THE ADRIATIC'
From an old book of travel, probably 18th century

PLATE III



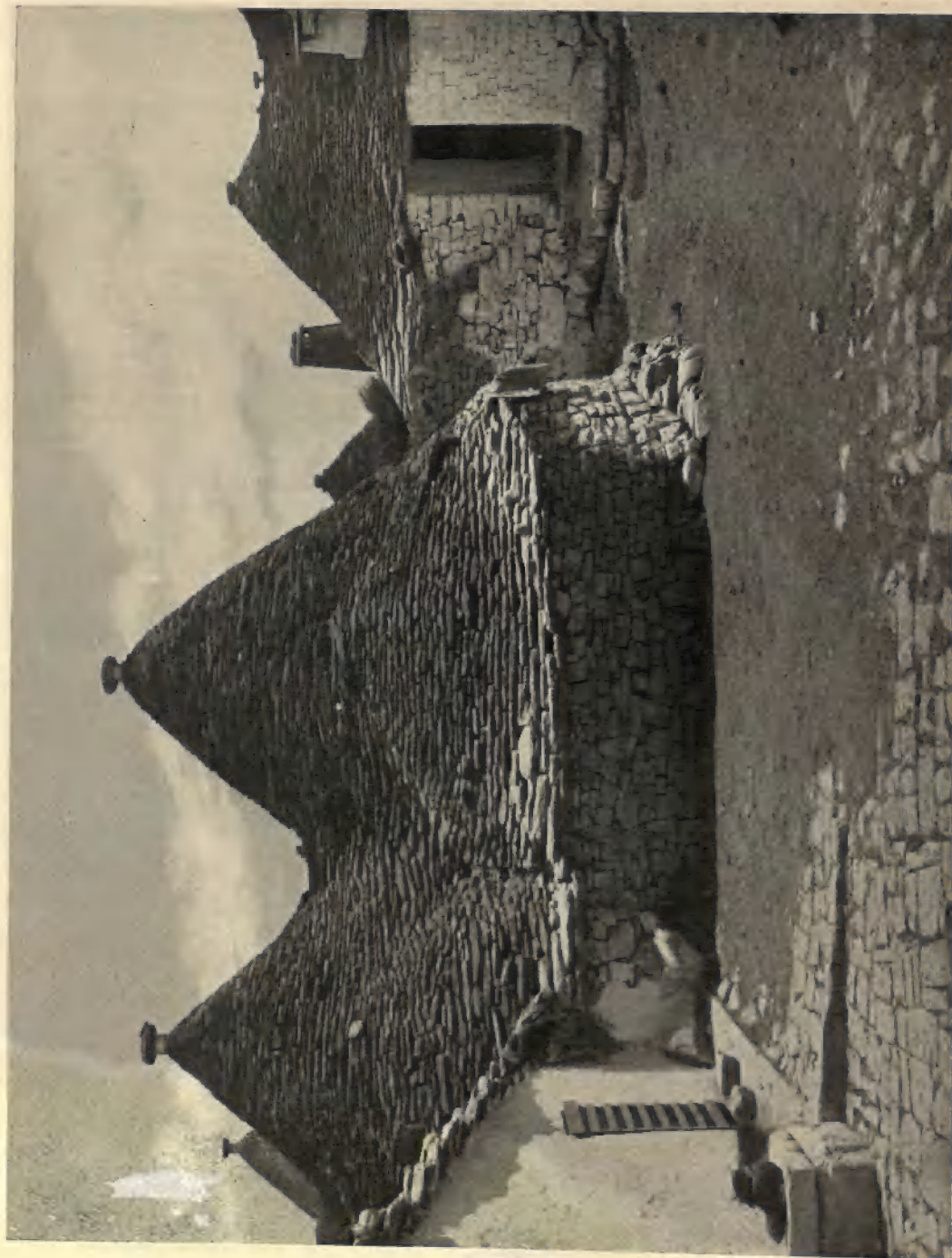
'TRULLI' DWELLINGS, ALBEROBELLO, ITALY
Pls. III-VI, *ph.* Alinari

PLATE IV



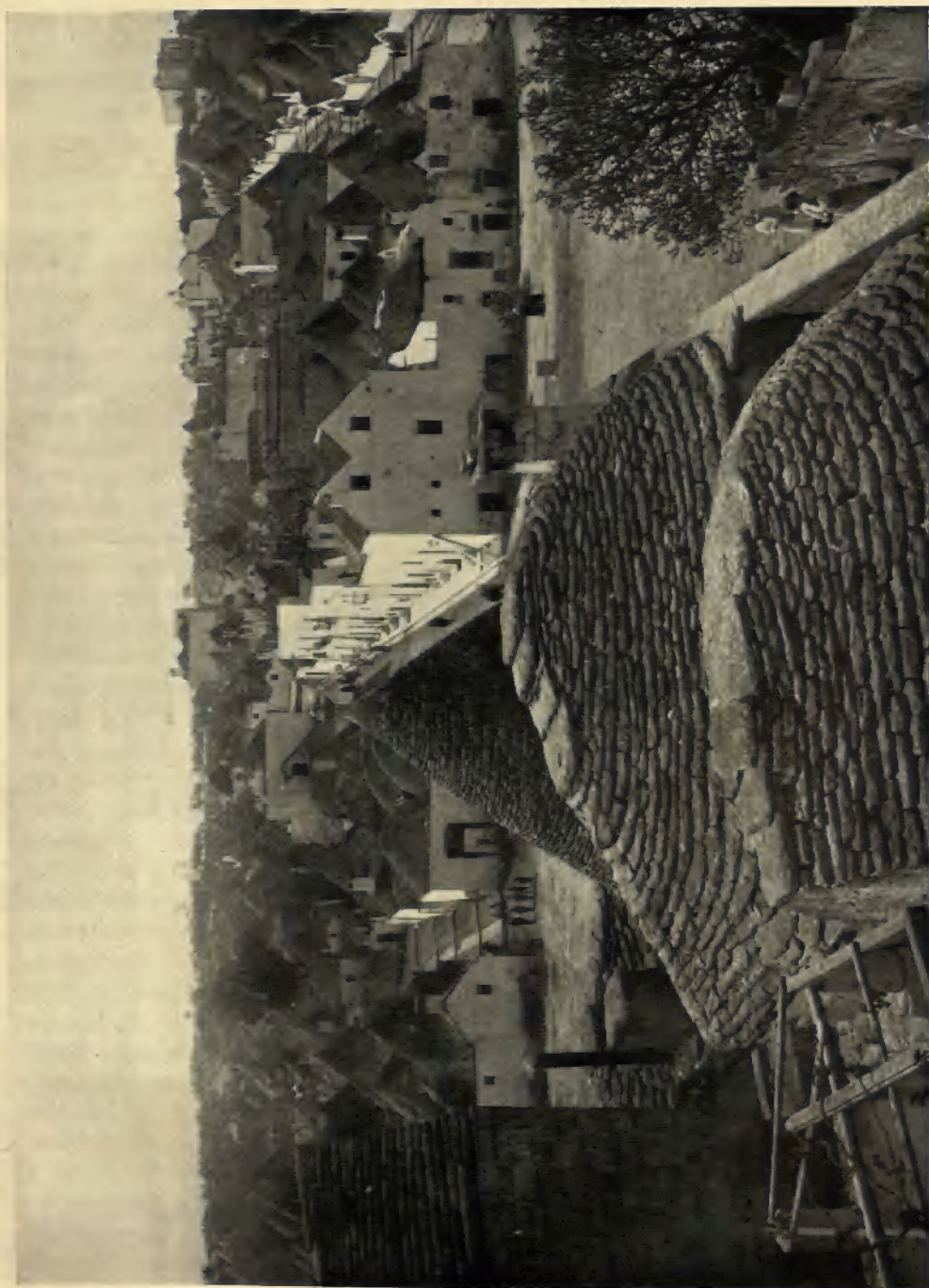
'TRULLI' DWELLINGS, ALBEROBELLO

PLATE V



A 'TRULLO', ALBEROBELLO

PLATE VI



GENERAL VIEW OF ALBEROBELLO

BEE-HIVE DWELLINGS OF APULIA

possible. Walls of 7 or 8 feet thickness are a convenient way of disposing of a surplus, said he. Such economical houses, too, he added, for the material costs nothing, not even the expense of carting, for the builder is sure of finding stone wherever he may wish to build. And by building, he is helping to clear the land, and thus placing more of the rich soil at his disposal for cultivation. Construction costs nothing but time, for each peasant builds his own trullo, with the aid of his neighbours if need be. Architects and masons would fare badly in Alberobello. I learnt that the town possesses neither.

Some interesting variants appeared as we wandered along. I saw a few trulli of more recent construction which consisted of two stories, access to the top floor being gained by a ladder fixed to the wall. Cellars also had been provided in some cases—deep, roomy cellars, cleverly fitted with partitions for wine and oil. One owner had a diminutive trullo adjoining, which housed families of white rabbits and chickens. In no case did I see an instance of more than one door, nor what could fairly be described as a window, although a few possessed holes in the walls some 15 inches square, left open in the summer, to be closed with stones in winter. The inhabitants all assured me that a trullo is an ideal dwelling, in that it is cool in summer and conserves the heat in winter—a most important factor in a land where coal is unknown, and wood scarce.

The builder of a trullo has little opportunity to express individuality ; indeed it is only into the question of decoration that personal taste can enter. Many of the houses are adorned with paintings above the door—vivid scenes from the Bible or local history, such as are to be found on the carts in Sicily. The apex of the roof is another spot where the artistic builder lets himself go, the lump of plaster which is the normal terminal being often varied by a sphere, a cross, an inverted jar, or, as in one instance, by the image of the patron saint of the family who dwell below. But as nature has provided the material wherewith to build the trullo, so it is the same kindly nature who has added the most fitting adornment. For in course of time those mortarless stones have become receptacles for soil and seeds driven in by the winds. The rain and the sunshine have done the rest, with the result that most of the trulli possess charming roof-gardens, aflame with masses of wild flowers, a delight to behold.

Scientific investigation seems to have little to add to what the natives know already about their extraordinary town. The distinguished Italian antiquary, Prof. Giuseppe Grassi, describes it as 'extremely

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ancient and certainly pre-historic', and his compatriot Prof. Bertacchi declares the trullo to be 'a survival of the remotest period'. As for the word itself, its origin has not been satisfactorily determined. It may be, as some think, that it has some affinity with 'trullus'—the domed chamber in the ancient Byzantine palace at Constantinople which is certainly after the same pattern—but in view of the undoubted prehistoric origin of Alberobello, this derivation seems fanciful and unlikely. Better, I think, fall back on the explanation given by my native rustic: 'The word means, well, it just means what you see here'—and his guiding hand described a complete circle.

It is painful to have to add that modern 'civilization' has reached Alberobello. No one will grudge it the blessings of electric light which it now enjoys like every other Italian town and village. And the recent arrival of a branch of a well-known firm of sewing machine manufacturers will doubtless be of service to the women, who have long been famous for their skill at needlework and weaving. But one cannot but regret that the 'High Street' of Alberobello already boasts of a cinema and is threatened with another. I like to think and believe that long after those cinemas have decayed to dust; ages after those sewing machines I saw being unswathed from their wrappings have gone to that limbo which awaits all sewing machines, Alberobello and its trulli will still remain as the permanent monument to the industry and skill of those prehistoric builders who had to build before they could plant, and who by their building enriched the world with a city at once fantastic and unique.

Dogs

by Dr MAX HILZHEIMER*

I DO not intend in this paper to give a complete history of the dog, but simply to discuss certain problems connected with it, and in particular to describe a number of types belonging to different periods and countries.

Our evidence is for the most part pictorial; yet it must be used with caution, for difficulties are bound to arise if animals are represented



FIG. 1. MALTESE DOG, VASE FROM VULCI
(*Zeitschrift für Hundeforschung*)

alone, with no clue to their size. For example, figure 3 shows representations of dogs on coins from Panormus of the 4th century B.C.¹; artistically they are excellent, but nothing can be inferred as to breed, for this depends on the size, of which we know nothing. On the other hand, figure 1, although artistically crude, not only tells us the size of

* This article originally appeared in German in the *Zeitschrift für Hundeforschung* (Berlin), April 1931, pp. 3-14, and is printed here by permission of the Editor and the Gesellschaft für Hundeforschung. It has been translated, with annotations, by Roland G. Austin, of Glasgow University.

¹ See Imhoof-Blumer and O. Keller, *Tier- und Pflanzenbildern auf Münzen und Gemmen des klassischen Altertums*.

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the dog relative to the man beside him, but also that the animal is of Maltese breed² (ΜΕΛΙΤΑΙΕ). This is in fact the only instance in antiquity where a breed can be definitely ascertained, for even when descriptions are given of classical breeds, they are as a rule thoroughly unsatisfactory and establish no certain conclusion.

I believe that all breeds of dog, with the single possible exception of the Egyptian and its derivative the greyhound, have been evolved ultimately from the wolf. From the skull-formation of different dogs one can recognize a point of arrested development at different stages of growth. Figure 4 shows three stages in the skull-development of the sheep-dog, illustrating the difference between the new-born puppy and the mature animal—a difference so striking that no one could imagine both to belong to the same breed were the two skulls found independently with no knowledge of their relationship. It can be seen that the skull of the month-old dog resembles that of the full-grown toy terrier, while at 6 months it is more like that of the pomeranian or the terrier. When completely developed it reaches a stage through which the mastiff has to pass before attaining maturity; and the fully-grown mastiff in its turn corresponds merely to one of the earlier stages of the wolf, which develops to a proportionately higher degree.

No domestic dog or breed betrays the fox in the shape of its skull; for in this respect the fox and wolf differ completely. In fact the fox has no place in the genealogy of the house-dog, even though many dogs are superficially very like foxes.

If the wolf is the ancestor of the domestic dog, an accurate knowledge of his build is of the greatest importance in the case of dogs broken in for use (*Gebrauchshunde*). The wolf is the most completely developed type of the trotting dog; no dog can outstay him when he trots. He is remarkable for his sharp-cut hindquarters, his long flexible spine, the firm set of his shoulders, and his keel-shaped breast with its slight development in point of depth. Owing to the narrowness of the shoulders the elbows are turned well inwards; thus it happens that the radius-bone reaches forward and outwards, and so he trots very powerfully, with the hinder feet always keeping to the tracks of the corresponding forefeet, unlike the dog, whose hinder tracks are to left or right of those made by the forefeet. Another indication of

² These dogs, referred to by Strabo (VI, 277) as τὰ κυνίδια ἃ καλοῦσι Μελιταῖα, were frequent domestic pets; see Daremberg and Saglio, *Dictionnaire des Antiquités*, s.v. *bestiae cicures*, figs. 834 and 840, and fig. 1113 s.v. *canis*.

DOGS

the tireless runner is the strongly developed nose with its straight bridge and the sloping brow (fig. 5).

The Australian dingo is another type of wild dog; he stands some 40-50 centimetres high at the shoulders, and is usually of a reddish colour. But it is open to doubt whether he was really wild in origin. Apart from certain mice and bats, which may have reached Australia on driftwood or by flying, there are no taller mammals to be found there, and it is therefore thought that immigrants were originally responsible for the introduction of the dingo and that he became wild later. There is much to be said for this opinion, for the dingo is definitely a dog in build, even though he has the rather long back and tiny slit eyes of the wild dog. However, dingo bones have been found along with those of extinct beasts of prey, so that there is at least some support for the view that he is a genuine wild dog.

Studer is the chief exponent of the theory that a smaller type of wild dog must have existed south of the zone where the wolf is found, and that its habitat possibly extended to Australia; but so far his view lacks proof, unless the pariah dogs, so numerous everywhere from Morocco to Japan, are to be considered as descendants of such an animal.

Here again we come to a difficult problem: is the pariah a wild dog raised to the status of a domestic dog, or is it rather a runaway domestic dog which has become half-wild? I prefer the latter view. For pariah dogs are found nowadays in countries which had reached a high cultural development in antiquity, yet nothing is heard of these animals at that time. On the other hand it must be remembered that it was precisely these countries which were overrun and laid waste by invading peoples at the dissolution of the ancient world; it is therefore possible that many dogs then became homeless and turned pariah. If I am right in assuming that the pariahs have evolved from a casual cross-breeding of very different types, we might perhaps discover in them a prototype of a dog (*Urtyp*) in much the same way as Darwin, by crossing different breeds of domestic pigeon, discovered its racial ancestor, the wild rock-dove. The pariahs are thus of greater interest, and it is regrettable that their origin has never been carefully investigated; in fact there is grave danger of their becoming extinct before any such research can be made. In spite of their similarity, different types may be recognized among these pariah dogs. They are of a medium size, standing about 50 centimetres high, usually of a reddish or yellowish colour; they vary from being smooth to wire-haired,

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with long muzzles, sloping brow, erect ears with the points often drooping, and the tail more or less tightly curled. In build, they range from a very heavy, lumpish pomeranian to the hound or sheep-dog types, which can even approach very nearly to the light build of the greyhound.

In this way these types represent so to speak a primitive stock such as that from which permanent types not unlike our hounds and sheep-dogs could have evolved. Studer, in his *Canis familiaris putiatini* from the Russian Campignian, claims to have found a common ancestor for both of these (the Campignian period follows the Azilian, which



FIG. 2. FRESCO (HELLADIC III) OF BOAR-HUNT AT TIRYNS
(Zeitschrift für Hundeforschung)

may be placed immediately after the Ice Age). Although his *Canis familiaris putiatini* has lately been again subjected to scepticism, there is no doubt of the close connexion between the hound and sheep-dog. Figure 6 shows a dog from Tripoli, which happened to be exhibited in an international show at the Berlin Zoological Gardens ; from it a clear idea can be obtained of their common ancestor. I find it difficult to find a place for this dog in any modern system of classification of domestic dogs, whereas his type appears to me to be frequently represented in ancient mural paintings and on vases. For instance, he is exactly like the dogs in the fresco of a boar-hunt at Tiryns, dating from about the 14th century B.C. (fig. 2). Clearly the breed spread over the eastern Mediterranean, and has remained quite pure in Tripoli down to the present day.

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While the hound and the sheep-dog belong to a relatively recent period, two other breeds occur in the oldest strata containing evidence of the domestic dog—the kitchen-middens of the Baltic and North Sea coasts, which in geological terms belong to the immediately post-glacial period, and culturally correspond to the Azilian period.

Of these, the larger and older so obviously resembles the wolf in its skull-formation that there can be no doubt of its descent, especially since other remains have been discovered which according to Brinkmann may belong to a domesticated wolf. This is the *canis familiaris Inostranzewi*, which is thought to be the ancestor of the great mastiffs and shepherd-dogs, and which Brinkmann has shown to have a living representative today in the Norwegian elkhound. The breed early spread southwards from the Baltic coast, their original home. Thus in Babylonian art about the close of the 3rd millennium B.C. we come across a gigantic mastiff about 80 centimetres shoulder-high (fig. 7).³ Today a quite similar breed seems to survive in Central Asia; in 1930 at the international fur-trade exhibition I saw an animal among the Turkestan shepherd-dogs which reminded me at once of the early Babylonian mastiff. Of course there were other examples of these dogs, smaller and slighter, showing that the breed is not so entirely stabilized as those systematically bred in countries with a high degree of culture. So that it is not surprising that in ancient Mesopotamia there were also smaller dogs 60 to 70 centimetres shoulder-high, used for hunting and perhaps for purposes of war as well. In the first half of the first millennium B.C. dogs were employed in campaigns all over western Asia and Greece; figure 8 shows them in action (war-dogs of Assurbanipal from the palace of Nineveh, about 650 B.C.). Herodotus⁴ recounts that when the Perinthis were attacked by the Paeonians on the Propontis, 'man was matched against man, horse against horse, dog against dog'. The same use was made of dogs by other nations also, and no doubt the breed employed varied with the respective peoples. The Assyrian war-dogs appear very different from the animal represented as accompanying Hecate on the frieze from Pergamum (figure 9;

³ This is now in the British Museum and is described there as a hunting-dog; cf. W. Houghton, in *Trans. Soc. of Biblical Archaeology*, v, 53; Birch, *History of Ancient Pottery*, I, 147.

⁴ Herodotus v, 1; cf. Aelian, *de natura animalium*, vii, 38, where the Magnesians and Hyrcanians are said to have the same custom; Aelian adds that an Athenian soldier took his dog into action at Marathon, and that the two together were depicted in one of the paintings on the Stoa Poikile.

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first half of the second century B.C.), a shaggy hound with short muzzle and ears erect, biting a giant on the leg. The Gauls clad their war-dogs in armour, with broad spiked collars. The Romans, however, did not employ dogs for purposes of war, although as Vegetius⁵ tells us, writing about A.D. 400, they kept numerous dogs in their watch-towers on the *limes*, to bark on the approach of enemies. Dogs were also used for carrying messages, with letters attached to their collars.⁶

Egypt is the most southerly point reached by this mastiff-type in prehistoric times. We can trace them in the period of the first dynasty, about 3000 B.C. They probably accompanied nomad foreign tribes thither, and soon became extinct from lack of further stock. They must have resembled the huge Babylonian dog, from which they were most probably descended, and were not much smaller than the lions often depicted with them.

The great shepherd-dog (*Hirtenhund*) is also usually connected with the short-haired mastiff type mentioned already. Care should be taken not to confuse him with the sheep-dog (*Schäferhund*). The former were used not for taking the herds to and from the pasture, but in order to keep guard against robbers of all kinds, human and animal.⁷ The latter type as we know it can only have been evolved after the gradual disappearance of great beasts of prey, more especially the wolf, and when the spread of agriculture necessitated the 'watch-dog' in the modern sense. There is no proof of such sheep-dogs in general use before the 17th century, when they first appeared in England as the wolf became extinct.⁸ For although the zoological category of 'sheep-dog' is known to have existed from the Bronze Age downwards, these animals originally served very different purposes, such as hunting or keeping guard.

These great shepherd-dogs may be divided into two groups. One is found south of the Alps, with long lightly waving hair (*e.g.*, the Pyrenean type or the south Italian). This breed is very ancient and is depicted in Roman works of art, being especially frequent in the Alpine districts, where it probably was the forerunner of the modern

⁵ *Res militaris*, IV, 26.

⁶ Aeneas Tacticus, xxxi, 32 (of the Epirotes and Thessalians). Varro (*de re rustica* II, 9, 15) recommends spiked collars (*mellum*, *millus*) as a protection for sheep-dogs; cf. Rich, *Illustriertes Wörterbuch der römischen Altertümer* (1862), p. 395.

⁷ *i.e.*, the *canis pecuarius pastoralis*; thus Plato (*Rep.* 416A) speaks of them as *ἐπίκουροι ποιμνίων*; cf. Daremberg and Saglio, s.v. *canis*, figs. 1118 (bas-relief from the Capitol), and 1119 (Greek vase, Musée de l'Ermitage).

⁸ cf. J. E. Harting, *British animals extinct within historic times*, pp. 115-205 (the wolf).

PLATE I



FIG. 3. COINS FROM PANORMUS, 4TH CENT, B.C.
(British Museum)

PLATE II



FIG. 4. STAGES OF SKULL-DEVELOPMENT

A. Wolf, full-grown. B. Sheep-dog, full-grown. C. Pomeranian, full-grown. D. Sheep-dog, 6 months.
 E. Toy-terrier, full-grown. F. Sheep-dog, 4 weeks. G. Mastiff, full-grown.

(*Zeitschrift für Hundeforschung*)

PLATE III



FIG. 5. RUNNING WOLF
(Verein für Deutsche Schäferhunde, Augsburg)

PLATE IV

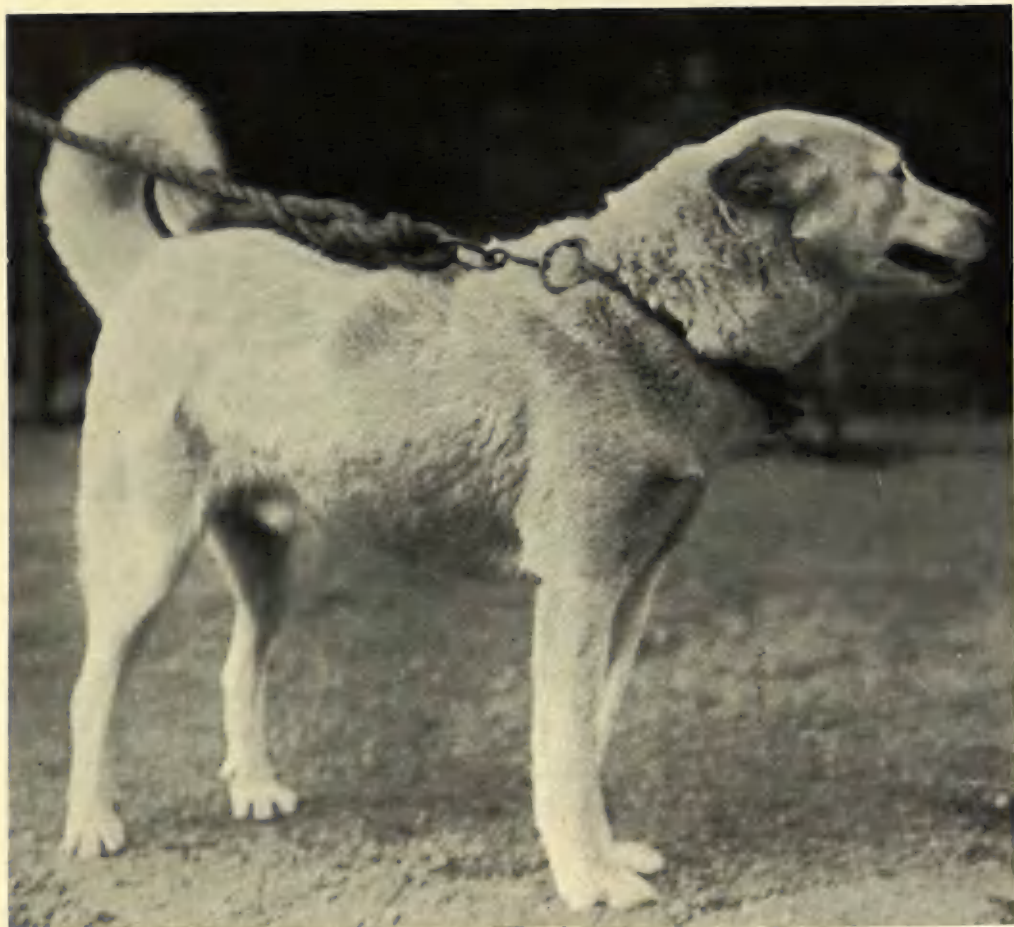


FIG. 6. TRIPOLITAN DOG
(Zoological Gardens, Berlin)

PLATE V



FIG. 7. MASTIFF, BABYLONIAN RELIEF, c. 2200 B.C.
(*British Museum*)

PLATE VI



FIG. 8. WAR-DOGS OF ASSURBANIPAL, PALACE OF NINEVEH. c. 650 B.C.
(British Mus.)



FIG. 9. ASSYRIAN WAR-DOGS, FRIEZE FROM PERGAMUM, 2ND CENT. B.C.
(*Direction des Pergamon-Museums, Berlin*)

PLATE VIII

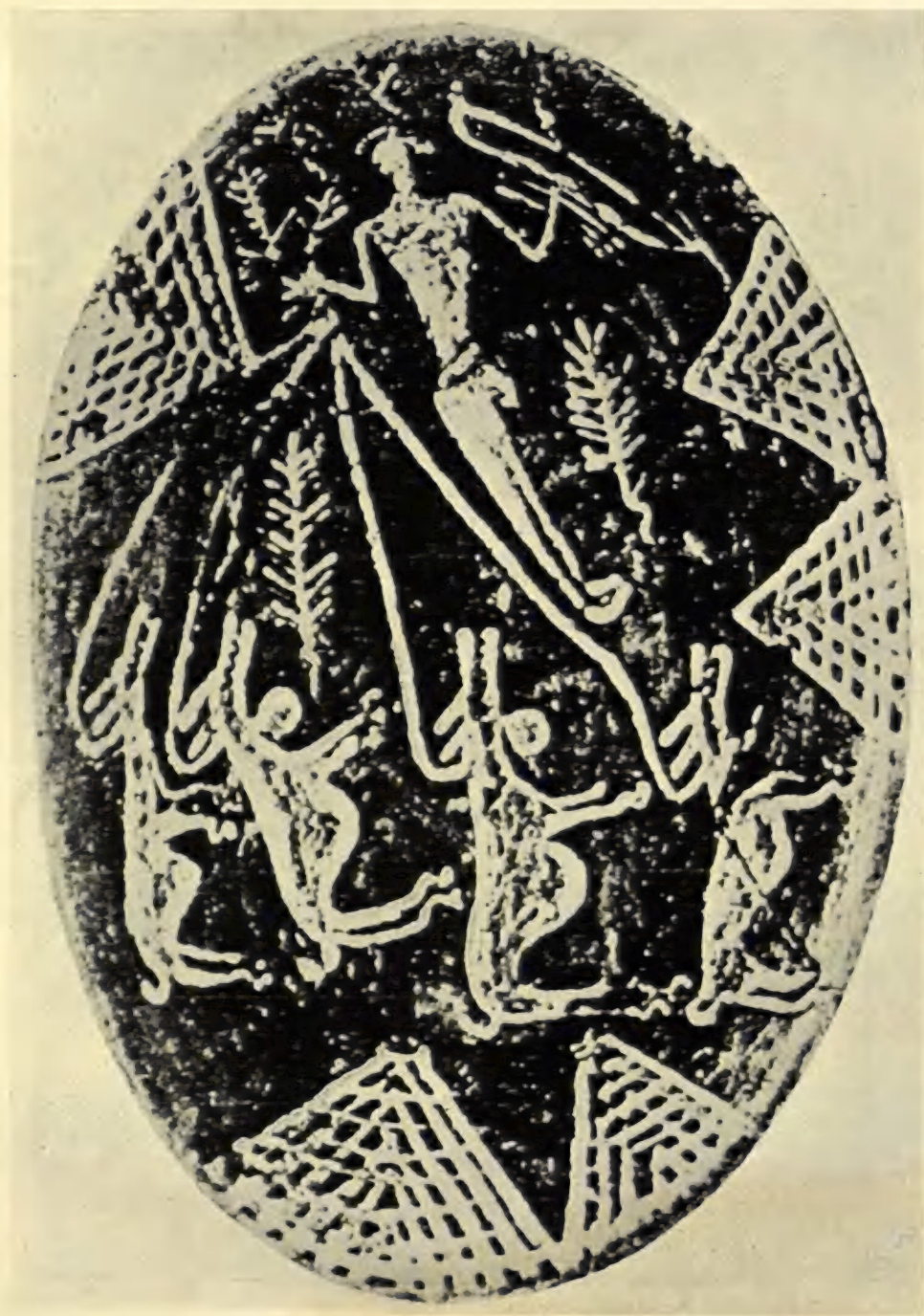


FIG. 10. EARLIEST REPRESENTATION OF EGYPTIAN DOG, PRE-DYNASTIC
(*Zeitschrift für Hundeforschung*)

PLATE IX



FIG. 11. EGYPTIAN GREYHOUND, GRAVE OF PTAHHETEP, SAKKARA, c. 2500 B.C.
(*Zeitschrift für Hundeforschung*)

PLATE X



FIG. 12. IBIZAN GREYHOUND (BALEARIC ISLANDS)
(Zoological Gardens, Berlin)

PLATE XI



FIG. 13. EGYPTIAN RACING HOUND
(Ashmolean Museum)



FIG. 14. PAINTED EGYPTIAN BITCH, FROM BENI-HASSAN, c. 1900 B.C.
Drawing by Percy Buckman
(Exploration Society)

PLATE XII

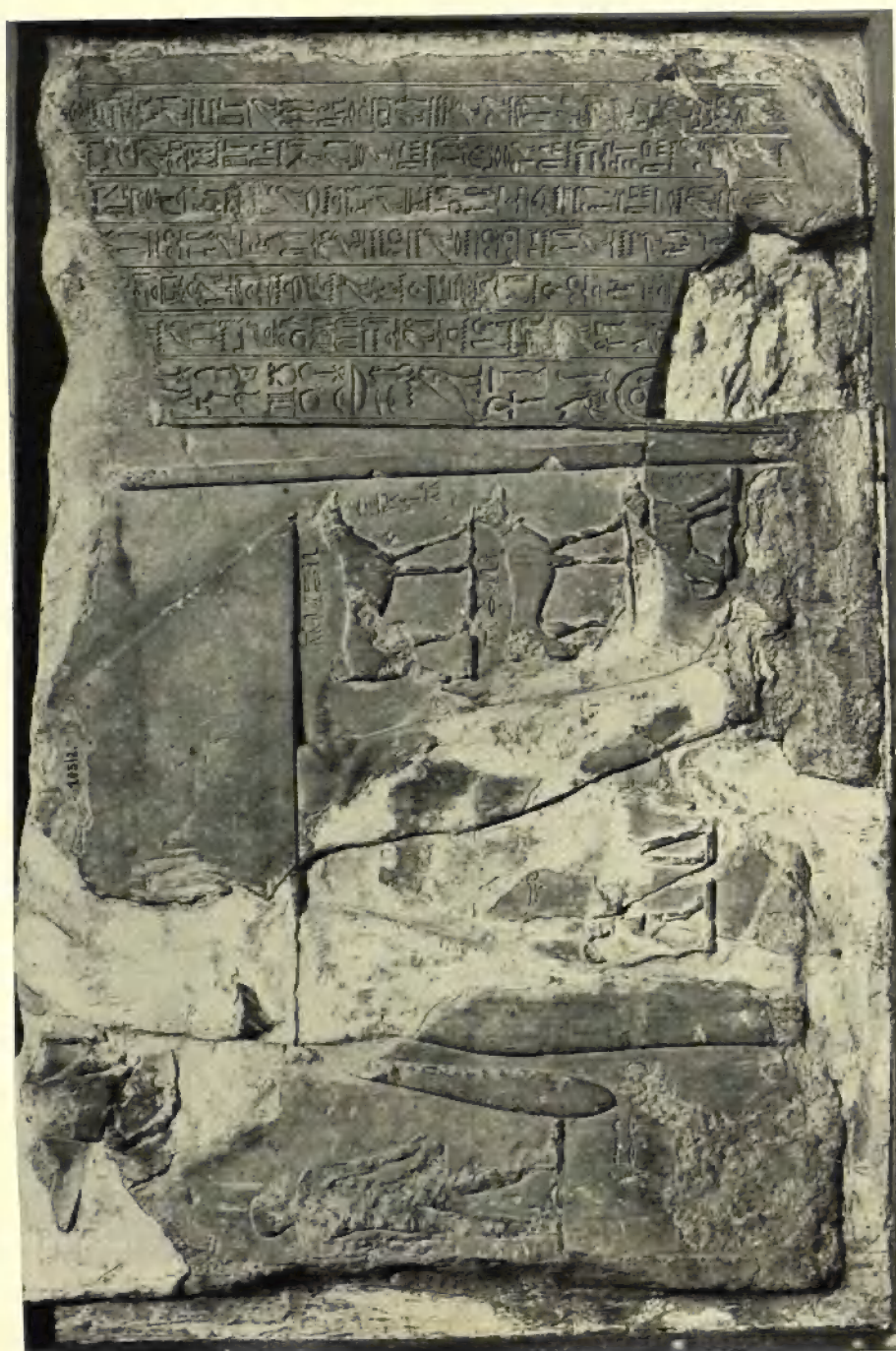


FIG. 13. STELE OF KING ANTEF, WITH HOUNDS, c. 2100 B.C.
(Cairo Museum)

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St. Bernard dog. The second group belongs north of the Alps, with tight corkscrew-like curly hair, resembling the Oftscharka, the old German sheep-dog from Pomerania, and the Bobtail. The prevailing colour among these dogs is white, if not pure white.

From several considerations it is erroneous to connect this breed with the so-called Tibetan dogs, which are mostly black or black with black-and-tan markings, and so, apart altogether from their skull-formation, very different in appearance. We know little about this breed. Its various representatives, shown in Europe during the last seventy years, offered a variety of types, and nothing is known of their history.⁹

A second group of dogs, dating back as far as that of the mastiff-type, comprises the fox-terrier and his close relation the terrier. Their earliest representatives occur in the Azilian period on the coast of the Baltic and North Sea. They seem to have spread over the world quite early, the more because they can resist climatic conditions, especially hot temperatures, much more than the others. They had reached South Africa and had spread over the Polynesian islands before their introduction into Europe. In the early Stone Age they make their appearance in the prehistoric lake-dwellings of Switzerland, and about 3000 B.C. may have reached Mesopotamia, as shown by a seal of the period, on which there is an undoubted representation of a Pomeranian; this by the way is the only illustration of a dog of this group known to me from ancient Mesopotamia.

These dogs do not seem to have reached Egypt so early. The earliest representation of an Egyptian dog occurs on a pot belonging to the pre-dynastic period, dating from about the fourth millennium B.C. (fig. 10). It depicts a man with four dogs on a lead. From their build the animals can only be greyhounds, of the size of whippets, with the erect tail characteristic of their kind. They are probably to be considered as representatives of that type of greyhound later so highly bred in Egypt, which were called there Tesem (fig. 11, early Egyptian greyhounds from the grave of Ptahhetep at Sakkara, belonging to the fifth dynasty, about 2500 B.C.). We possess many illustrations of these Tesem greyhounds from the whole period of the Egyptian supremacy, some 3000 years, and much care must have been taken to

⁹ O. Keller, *Die antike Tierwelt*, suggests that these were the 'Chinese' dogs mentioned by Grätius, *Cynegetica* 159 (sunt qui Seras alant, genus intractabilis irae); he states that according to Chinese chronicles one of these Tibetan dogs was presented to the reigning emperor in 1121 B.C.; but there is nothing to show that they originated in China. (Keller, p. 108-9, where there is also an account of the Indian dog).

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keep them pure-bred. They are always depicted with long, pointed, fine heads, ears stiff and sharply erect, and a tightly-curved tail like that of a pug. According to Egyptian ideas this curly tail must have formed a sign of good breeding, for it is always carefully represented in all works of art, often to an exaggerated degree. Obviously the Egyptians were especially proud of this characteristic, quite unnatural in a greyhound, who needs a long tail to guide himself; it must therefore have been extremely difficult to obtain by breeding. This breed has died out in modern Egypt, but is still extant in Crete, the Balearic islands, and Pityusa, as Conrad Keller first pointed out. Here the island of Ibiza is the chief breeding-centre. The Spaniards have lately introduced this 'Mallorquin' or Ibiza greyhound to the Canary Islands, and the Berlin Zoological Gardens possess at the moment a male and female specimen, presented by the well-known traveller Spatz (fig. 12). In several respects these dogs appear to me unlike the original Egyptian type of greyhound in build. I imagine the latter to resemble the Borzoi-type; the back of the Borzoi has a much more pronounced curve, and the proportion of upper hind-leg to lower, and similarly that of upper and lower foreleg, seems to me to be different. This would have to be ascertained by investigating the bone-structure. But in particular the skull-formation of the two seems to differ completely. The head of the Balearic greyhound appears remarkably hollow in front of the eyes. In this way, and not only because of the peculiar square-shaped erect ears, the head is entirely different from that of the Eurasiatic greyhound. This is best seen from the profile: in the latter type the part before the eyes is more or less convex, in the African dog unmistakably concave. Of course, this conformation has nothing to do with a higher development of the brain, but depends upon the hollows of the brow, which extend from the nose to the frontal bones. I have proved the same peculiar skull-formation to exist in early Egyptian mummified dogs, which I should like to think belong to the Tesem breed. On the other hand these mummified skulls correspond very closely to one of the three Egyptian types of jackal, the *canis lupaster*. Further, as we have seen, these greyhounds are of great antiquity in Egypt, and appear to have existed at a period when there is no trace of the greyhound in Asia or Europe; and I believe therefore that the Tesem represents a very ancient Egyptian breed. My opinion is strengthened by the fact that the ancient Egyptians were complete masters of the art of domestication, and tamed a large number of animals which were not so elsewhere.

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Besides these greyhounds, there appears to have existed quite early in Egypt another breed of racing dog, which can be proved to go back at least to before the first dynasty. These dogs had long, thin legs, small floppy ears and sword-like tails; figure 13, an ivory figure from Hieraconpolis dating from the end of the 4th millennium B.C., gives a good idea of their appearance. The breed continued throughout the whole period of the Egyptian supremacy and is well attested by many works of art, though we do not know its name.

At a later period, especially in the new Empire, a large number of types made their appearance, which are not always easy to describe in detail, and may partly have been mongrels. Attention has been especially drawn to a type of low-slung dog with erect ears; his straight legs notwithstanding, he has been thought to be the ancestor of our dachshund, and an attempt has even been made to base this claim on linguistic grounds. In the 11th dynasty (about 2100 B.C.) King Antef is represented with his dogs (fig. 15), and against these is written a word which has been read as *tekal*; this word has been identified with the low German word for dachshund, *teckel*. Nothing can be further from the truth than this derivation. Granted that the ancient Egyptians had a word *tekal*, even with the most reckless philology it could not be connected with the low German *teckel*. However, the word in the inscription is actually *trqu*, which means something hot or fiery, and must therefore be the name of the dog and not a designation of its breed. Apart from the fact that the four dogs of King Antef, each of which has another name, are especially lean and long-legged, they resemble the greyhound in type, perhaps even the racing type, but in no case the Dachshund (v. fig. 15). There is nowhere any name attached to representations of these low-slung long-bodied dogs, as they appear for example in those from the buried Egyptian city Beni-Hassan (fig. 14, dating from the 12th dynasty, about 1900 B.C.).

As for the descent of the dachshund, in my opinion it is an entirely German breed, which sprang up about the time of the Roman empire in that part of Germany which was under Roman domination. In every Romano-German settlement belonging to that period which has yielded remains of domestic animals—and I have investigated a large number—I have found remains of the dachshund. Other researches may have led to the same discovery. As they are so far not known from earlier cultural strata or in other localities, my conclusion seems to be well founded: that Germany is the cradle of the dachshund, and that it must be regarded as an ancient German breed.

Alleged Norse Remains in America*

by G. M. GATHORNE-HARDY

THE discovery of America by the Norsemen in the early years of the eleventh century is now one of the accepted facts of history. While considerable difference of opinion still exists as to the measure of reliance to be placed, in points of detail, upon the documents in which the discovery is recorded, and the precise localities visited are still the subject of controversy, no one who has devoted any attention to the subject entertains any doubt that the broad fact of the discovery is established. But, though this is so, the evidence has hitherto been exclusively confined to the historical narratives of the sagas, and the attempts which have been made from time to time to supplement their testimony by the production of traces of Norse occupation discovered in America itself have not only broken down, but have excited so large a measure of ridicule and suspicion that they have, rather paradoxically, been the principal reason why general acceptance of the basic fact was unduly delayed. As a natural and justifiable result, the first reaction of students of the subject to alleged discoveries of Norse remains or inscriptions in the West is one of extremely cautious scepticism.

The question has recently been revived by the publication of a work¹ which demands at the very least a serious and dispassionate consideration. Mr Holand's attempted vindication of the claims of the Kensington inscription has involved the continuous application of more than twenty years; the objections raised by its opponents appear to be frankly and fairly disclosed and faced; the reasoning of the book is close and forcible, and, since the issues involved are those of deliberate fraud and perjury, the problem has all the fascination of a detective story or a *cause célèbre*. In any paper devoted to reputed Norse remains in America, the Kensington Stone is the only instance deserving serious discussion, but, having regard to the atmosphere

*References to America in this paper distinguish Greenland, to which different considerations apply.

¹ H. R. Holand, *The Kensington Stone*. Ephraim, Wisconsin, 1932. 3 dollars.

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created by previous allegations of the kind, it is desirable briefly to summarize the general history of the question.

It may at first sight seem surprising that the visits of the Norsemen should not have left behind them abiding monuments sufficiently conspicuous and widespread to have been discovered by this time. If, indeed, it were merely a question of the recorded voyages of Leif, Karlsefni and others in the early part of the eleventh century, there would be little reason to anticipate discoveries of this kind. Without a more precise knowledge than we at present possess of the regions visited by the first explorers, any traces which they may have left of their transitory presence might as easily escape notice as the proverbial needle in a haystack. But there is indisputable evidence of the fact that knowledge of the American discovery persisted in the Norse colony of Greenland for several centuries, and there is every reason to believe that the country was the object of frequent visits, if not of attempts at permanent colonization. The Icelandic Annals record how, in 1121, Erik, the first bishop of Greenland, sailed to Wineland (America), from which he appears never to have returned. This in itself is rather suggestive of the existence at that date of a Norse settlement on the American mainland. It would hardly be the function of a bishop to desert his diocese with the object of founding such a colony, a motive suggested by the imaginative Danish poet Lyschander in the seventeenth century; still less would he be likely at that date to attempt the hopeless task of a mission to the barbarous 'skraelings'. On the other hand, if a portion of the flock entrusted to his charge were known to have emigrated to the more congenial climate of Wineland, the motive of the bishop's journey is at once intelligible. However this may be, there can be no doubt that to a far later date connexion between Greenland and America was maintained.

In 1347, The Annals make a casual reference to the arrival in Iceland of a Greenland ship which had sailed to Leif's Markland, and the unique character of this reference is clearly explicable from the circumstance that this voyage came accidentally to the notice of the Icelanders: ships plying between Greenland and America would normally have been out of the purview of the annalists. It is, incidentally, not without importance in connexion with the problem of the Kensington stone that this crew of Greenlanders is known to have visited Norway in 1348, in company with one Jón Guttormson. A further hint of the probable existence of a more or less continuous traffic between Greenland and America is found in a list of furs stated

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by Archbishop Walkendorff of Trondhjem to have been imported from Greenland. Several of these are not attributable to the Greenland fauna, and it has been therefore plausibly suggested that they may have been obtained from the American continent.

Peculiarly suggestive, though perhaps of inferior trustworthiness, is an entry in the supplementary Icelandic Annals written in Latin early in the seventeenth century by Bishop Gisle Oddson. The source of his information is not known, but it may have been derived from a record burnt in the cathedral at Skalholt in 1630, and appears in any case too circumstantial for an effort of episcopal imagination.

1342. Grönlandiae inhabitatores a vera fide et christiana religione sponte sua defecerunt, et repudiatis omnibus honestis moribus et veris virtutibus *ad Americae populos se converterunt*: existimant enim nonnulli Grönlandiam esse vicinam vel adjacentem occidentalibus orbis regionibus.

The last part of this entry makes it clear that the bishop is distinguishing between Greenland and what he calls America, and Nansen's suggestion that the reference is to an amalgamation with the Greenland Eskimo is therefore hardly tenable, even if it were not clear from other portions of the same document that Gisle Oddson held the prevalent view associating Greenland with Europe. That something of the kind suggested by the record may have happened to the Western Settlement, near Godthaab in Greenland, is confirmed by the report of Ivar Bardsen, who, visiting that district about this date, found the colonists departed, and their cattle running wild about the place. As Nansen observes, had Eskimo raided the settlement, they would certainly have slaughtered the cattle. Moreover the fact that the latter had survived untended in that inclement region points to the very recent departure of their owners. The apostasy of the Western Settlement, and its emigration to America about this time may therefore be considered quite probable, and it is not impossible that news of the event may have reached Norway in 1348, with the arrival of the Markland expedition. This has a bearing on the question of the Kensington stone, which will be referred to later.

The discovery of a runic inscription or other evidence of a like kind in America would not therefore be an extremely unlikely occurrence. It must be admitted, however, that all attempts to substantiate anything of the kind, with the possible exception of the Kensington stone, have hitherto failed disastrously. The ball was set rolling in 1837 by the publication of C. C. Rafn's *Antiquitates Americanae*, which first brought to the American public a widespread knowledge of the

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Norse discovery. Misled, most probably, by the unscientific enthusiasm of American correspondents, this distinguished Danish antiquary committed himself to a belief in the runic origin of the well-known Dighton Rock inscriptions, now recognized as Indian pictographs, and to the identification as a Norse building of a ruin since proved incontrovertibly to be the remains of a seventeenth century windmill. For some time afterwards similar wild attributions seem to have been quite common in the United States, though comparatively few of these established a sufficiently long-lived reputation to be capable of being readily traced at the present day. It will be remembered, however, that Lowell, as late as 1862, satirizes the craze in the *Biglow Papers*, and it must therefore have been far more prevalent than is ordinarily realized. The most prominent examples are—the Fall River skeleton, demonstrably Indian, which inspired Longfellow's well-known verses; the Grave Creek mound inscription, which may or may not have been a forgery executed by the exhibitor of the mound, but which, as it is certainly not runic, has no direct bearing upon our enquiry; the Monhegan 'inscription', discovered in 1856 and reproduced in Sir Daniel Wilson's *Prehistoric Man* (vol. 2, p. 178), which is clearly a specimen of natural glacial scratches upon a rock; the markings, possibly of human origin, discovered upon a boulder near Yarmouth, Nova Scotia, an illustration of which may be seen in at least three publications,² showing again not the remotest resemblance to a runic inscription; and, finally, Professor E. N. Horsford's claim to have discovered Norse remains—not, however, inscriptions—on the Charles River, near Boston. This claim is too absurd for serious consideration, and the same may be said of the other examples. With the possible exception, however, of the tiny oval tablet known as the Grave Creek mound inscription, which, if a fake, does not seem to be an imitation of anything Scandinavian, all these 'finds' have this in common, that they are *bona fide* if misguided attempts to read Norse work into something with a perfectly genuine existence; there is not the slightest suggestion of conscious fraud or forgery.

In a somewhat different category stands the runic inscription which came to light on the island of No Man's Land, near Martha's Vineyard, in 1926, some three years after Mr E. F. Gray, the author of a recent work on Leif Eriksson, had informed the owner of the

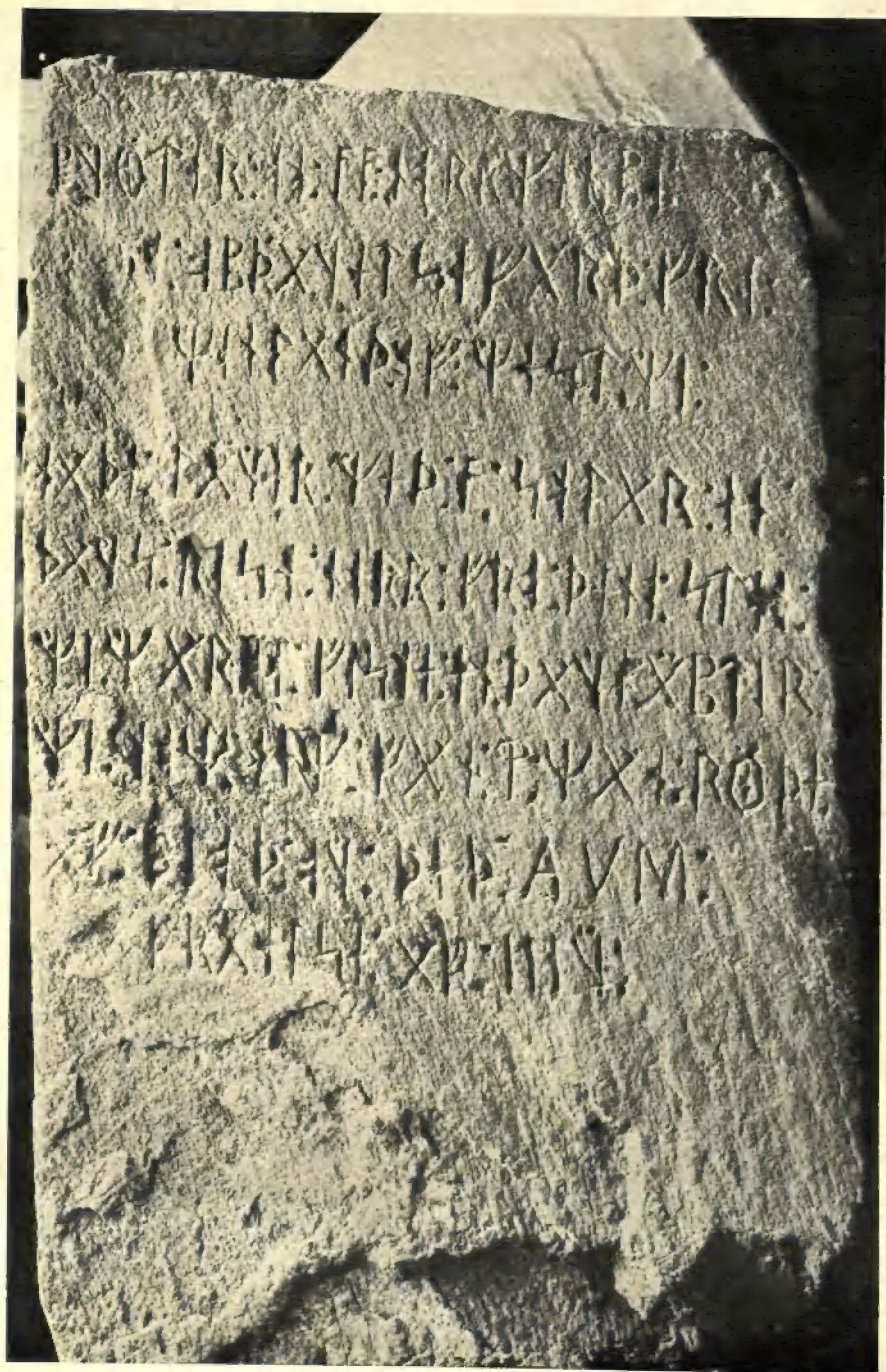
² *Procs. American Philos. Soc.* 1884, p. 491; Wilson, *The Lost Atlantis*, p. 54; Hovgaard, *Voyages of the Norsemen*.

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island that his investigations were trending in that direction. This is unquestionably a runic inscription of a kind, and equally certainly a fabrication. The most probable explanation of it seems to be that it was the work of a practical joker who wished to amuse himself at Mr Gray's expense. It does not appear to have been a serious forgery intended to deceive the world at large, and as a matter of fact it was so crude that Mr Gray was not tempted to place reliance on it, however opportunely it seemed to confirm his notion of the whereabouts of Wineland. He does indeed suggest that it may be a memorial to the Norse explorer set up by some seventeenth century navigator with inside knowledge, but this theory is too far-fetched to meet with general acceptance.

A possibly Norse origin has also been attributed, on several occasions, to certain stone ruins, on islands off the Labrador coast, which are assigned by the Eskimo to an extinct people of foreign stock, whom they call 'Tunnit'. I myself investigated a number of these remains in 1920. My own conclusions having been occasionally misunderstood or misrepresented, I should like to make them clear. In spite of certain peculiarities, such as the rectangular plan of some of the buildings, the ruins appeared to be the work of a very primitive people, of Eskimo culture, using stone implements, and I believe them to have been so. But this does not quite close the question, for, while the remains which I saw are of this character, I am still impressed by some remarkable features in the Eskimo traditions concerning the Tunnit, who are everywhere represented, right up to Baffin Land and consequently by a number of different tribes, as a people distinct from Eskimo, of large stature but few in numbers, who lacked such characteristic and necessary aptitudes as that of dressing seal-skins, and, though living by the sea and even on islands, had no boats save such as they could steal from the natives. These and other points incline me to suspect that the traditional Tunnit were quite possibly shipwrecked Norsemen from Greenland, and that some remains on the coast may yet turn out to be of such origin, but that the Eskimo of the present day erroneously attribute to them any unexplained traces of stone building, much as an English or Irish peasant will attribute to 'Caesar' or 'the Danes' any old remains, of whatever period. In the present state of the evidence, however, no connexion between the Norsemen and the Tunnit can be said to have been established.

Having cleared these preliminary matters out of the way, we may turn our attention to the Kensington stone, which anyhow raises



THE KENSINGTON STONE (FACE)

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problems of a much more serious character than have confronted us hitherto. The inscription in this case is either genuine or a deliberate forgery, and there seemed at first sight to be little difficulty in the way of adopting the latter hypothesis. The stone was discovered near Kensington, Minnesota, in 1898, and naturally the remoteness of the spot from any place associated with the Wineland voyages, together with the fact that the district was then as now settled by an almost exclusively Scandinavian population, at once raised a violent presumption against its authenticity. There seemed to be so many possible forgers with the necessary knowledge, and suspicion at once rested particularly on two—the finder, a Swede named Ohman, and a down-at-heel Swedish ex-pastor, named Fogelblad, who had died in the previous year. Fogelblad undoubtedly possessed a Swedish grammar by Almquist, containing the later runic futhork of sixteen characters, and illustrative selections from the Swedish language from A.D. 1200 to modern times. A further point accidentally contributed to throw discredit on the inscription. The date included in it was not for some time interpreted. Professor Breda, therefore, to whom the find was first referred, expected the language to be Old Norse of the eleventh century or thereabouts, and drew the obvious conclusion when he found instead something far more closely resembling the Swedish or Norwegian of the present day. The inscription was consequently regarded by him and others as a clumsy attempt to archaize modern Scandinavian speech, and the stone was returned in disgrace to the finder, in whose yard it lay disregarded till Mr Holand came to inspect it some nine years later.

‘Give a dog a bad name and hang him’. The disrepute which attached itself to the inscription from the first, whether deserved or not, places its champion at a serious disadvantage. Yet it is clear that the whole aspect of the case is changed if two points can be disposed of. If the stone purports, as it does, to belong to an age when Old Norse was no longer the appropriate language, one of the original grounds for condemning it disappears, and if it can be shown that the complicity not only of Ohman or Fogelblad, but of the whole Scandinavian settlement since its inception, is excluded, the question at once assumes a totally different complexion. It is to the latter point, which is of crucial importance, that Mr Holand’s first proofs are directed.

The defence takes the form of an alibi, and our final verdict will very largely depend upon how far we find this conclusive. There were absolutely no Scandinavians in the district before 1864, prior to

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which date, indeed, the hostile activities of the Sioux rendered it unsafe for white occupation. Little suspicion can attach to the first settler, a humble trapper, so it is not really necessary for the alibi to carry us so far back. The suspect Fogelblad first appears in the district in 1885; Ohman, the finder of the stone, in 1891. Now Mr Holand produces the sworn testimony of at least three independent witnesses, besides Ohman and his son, to the effect that the stone was found, in 1898, buried in the ground under the roots of an aspen, which grasped it in such a way as to show that the stone had lain there during the whole period of growth of the tree. The size of the tree, as described by these witnesses, varies from eight to ten inches in diameter at the base of the trunk. One witness, Nils Flaten,³ a neighbour, further deposes that he had visited the spot where the discovery was made earlier on the same day, and on many previous occasions, and had noticed nothing suspicious, and that, apart from the aspen, the ground was covered with a heavy growth of underbrush.

Now it is fair to point out that the evidence on the question was not sworn until many years after the original production of the stone, and also that, the tree itself having disappeared, Mr Holand's estimate of its age is based on an examination of similar specimens, corresponding as closely as possible to the descriptions given. It is more than likely that the witnesses may be mistaken in their recollection of the size of the aspen, and a considerable deduction may therefore rightly be made. The age of an aspen of the size described is, however, shown to be not less than seventy years, so that, when every legitimate allowance is made, it is still difficult to see how any of the modern Scandinavian population could have been responsible. Cut down by half from Mr Holand's figures, the alibi is still conclusive, unless we assume a deliberate conspiracy on the part of a number of apparently disinterested persons to commit perjury in support of a fraud with which it cannot be imagined that most of them had anything to do. Of course, if the evidence contains even a germ of truth, the case against Fogelblad and Ohman is smashed to atoms.

If we are driven to the conclusion that the inscribed stone was *in situ* before 1864, a conclusion borne out by the uncontradicted evidence of several geologists as to the weathering of the inscription, the hypothesis of forgery at once begins to appear improbable. A runologist in Minnesota at an earlier date would be a strange phenomenon,

³ Flaten, a Norwegian, had lived within 500 feet of the place of discovery since 1884.

FACE

Þ: 4011R: 14: FF: 11R R Y11: B1:

8 GÖTER OK 22 NORR MEN PÅ

1BBX41141PXRÞ: ÞR1:

OPDA GELSEF A RD FRÅ

4111X1Þ: 1P: 4111: 41:

WINLAND OF WEST WI

1XÞ1: 1X41R: 41Þ: F: 441X1R: 11:

HADE LÄ GER WED 2 SKJ A R EN

ÞX41: R141: 11RR: ÞR1: Þ111: 4111:

DA G S RISE NORR FRÅ THENO STEN

41: 4X1R: 14: Þ111: 11: ÞX41: X1Þ1R:

WI W A R OK F I SKE EN DA GH. ÄPTIR

41: 114: 114: ÞX1: Þ: 4X1: R1Þ1:

WI KOM HEM F A N IO M A N R Ö DE

XÞ: B1Þ1: 14: Þ1Þ: AVM:

A F BLOD OG DED A V M

ÞRX1141: XÞ: 1114:

FR Ä ELSE A F ILL Y

EDGE

1XR: Þ: 4X14: 41: 1X411: X1: 41:

HAR IO MANS WE HAWET AT SE

X1Þ1R: 41R1: 411B: 1F: ÞX41: R141:

ÄPTIR WORE SKIP 14 DA GH RISE

ÞR14: Þ111: 11: X1R: 1F1F:

FR AM THENO ÖH A HR 1362

NORMAL LATE RUNES.

ÞNÞ1 RY11 1141 BY11111YB111

F.U.TH.O. R.K.H.N.I.A.S.T.B.M.L.Y.E.D.G.P.Æ.Ö.

THE KENSINGTON INSCRIPTION, WITH TRANSLITERATION AND STANDARD RUNES FOR COMPARISON

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so strange, indeed, that he should be easy to identify. Having reached this point, it is time to consider the inscription itself.

In its original form, it is represented in the accompanying illustration, which also gives the transliteration from the runes. Translated, it runs as follows :—

8 Goths (Swedes) and 22 Norwegians on a journey of discovery from Wineland through the west. We had camp by two skerries (or possibly reef-like ledges on land—an alternative which Mr Holand perhaps unduly neglects) one day's journey north of this stone. We were (out) and fished one day. After we came home (we) found 10 men red with blood and dead. A.V.M. (Ave Maria or Ave Virgo Maria) Deliver (us) from evil (phrased according to the medieval and not the modern Swedish version of the Lord's Prayer).

Here the inscription on the face of the stone ends, though there is ample space for more. Down one edge, however, to exactly the same depth, it continues as follows, the surface of this part of the stone having been prepared for its reception by 'a rough bush-hammering'.

(We) have 10 of our men by the sea to look after our ships 14 days' journey from this island. Year 1362.

The situation of the inscribed portions suggests that the original intention was to set the stone upright in the ground, an unnecessary refinement for the purposes of a forger. The method adopted would have added not a little to the labour involved, which must in any case have been considerable, taking, it is estimated, not less than two days. In fact, the hypothetical forger has taken a great deal of superfluous trouble. The normal runic inscription plays but few variations on the precedent 'so-and-so set up this stone to so-and-so', and to have followed an easily accessible example would have been to play for safety and save time, while achieving the desired result of suggesting the early presence of Norsemen in the vicinity. But the supposed forger does not shrink from facing several more unnecessary difficulties. He goes out of his way to describe as an island a place which at the present day is nothing of the sort, and which does not even suggest that it was one in the past, except to the eye of a trained geologist. For the Icelanders and Greenlanders usually associated with Norse visits to America, he substitutes inherently improbable Swedes and Norwegians. He commits himself to the definite statement that he is fourteen days' journey from the sea, a statement manifestly absurd, if it is to be interpreted literally, even in the mind of the most ignorant settler in Minnesota. If we adopt for the sake of argument the popular theory that this forger is Fogelblad, he shows an even stranger perversity. He has, as we have heard, a grammar with a table of runes

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and specimens of fourteenth century Swedish. He makes no use of either. The u (v) sign, the second letter in his futhork, is ignored altogether. For the consonantal sound he substitutes a w, and for the vowel a y, the forms of each being apparently based rather on medieval ms than on any known runic character. The common runic k is also unknown to him, though, curiously enough, he knows (but reverses) the g, which is merely a dotted k. His sign for A is also unique, being closest in form to that found in a late Dalecarlian runic alphabet, which, however, in other respects is widely different. His ö is also unprecedented, and his d is invariably represented by the sign which the table in the grammar assigns to TH. On the linguistic side, the divergences of his grammar and spelling from accepted literary usage form the strongest weapon in the arsenal of those who attack the genuineness of the record. Fogelblad's possession of his grammar becomes, in fact, a strong point for the defence—no one with that grammar could have used those runes or perpetrated those solecisms.

So far, the hypothetical forger appears a first-class idiot and ignoramus. But he is not consistent in this character. Viewed from another angle, he shows himself a man of unusual scholastic attainments. The numerals used in the inscription are so unusual that for some time they baffled all attempts at interpretation, and even when their meaning was guessed it was thought, for years, that the system used was an invention of the forger. Eventually, however, it was found that they corresponded to a genuine medieval system, of which the only published account is to be found in a rare Latin work, written in 1643 by the Danish antiquary, Ole Worm. Even so, the examples given in this work are not slavishly copied; the same system is applied but with some significant divergences. Whence could a forger derive this recondite knowledge? Again, the phrase 'deliver us from evil' runs, in modern Swedish, 'fräls oss ifrån ondo', or, in Norwegian, 'frels os fra det onde', and we should expect a Minnesota settler to be satisfied with the familiar form of the quotation. But the inscription gives 'fräelse (os) af illy', which is phonetically to all intents and purposes identical with the form actually in use in Sweden in the fourteenth century—'fraelsae os af illu'.

The question of the date inscribed on the stone is also of cardinal importance. In any country, nine persons out of ten, interested in the Norse discovery of America, would have their minds concentrated on the early eleventh century, when the voyages of Leif and Karlsefni took place. The fourteenth century would never occur to them. If, on

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the other hand, the date, 1362, is to be regarded as a mere bow at a venture, drawn in complete ignorance, it is remarkable that it lands on the only date in the Middle Ages when the presence of a Scandinavian expedition in America—an expedition, moreover, composed of Swedes and Norwegians—can be shown to be probable.

As has been mentioned earlier, about 1342 there is reason to believe that a considerable number of apostates from the Greenland colony may have emigrated to America, and thrown in their lot with some tribe of Indians, with which they may have become previously acquainted through trading. News of this event may easily have reached Norway in 1348, when a number of Greenlanders are known to have sailed to that country. However this may be, it is certain that in 1354 King Magnus Eriksson, who ruled at that time over both Norway and Sweden, commissioned one Paul Knutson to sail to Greenland, with the special task of seeing to the religious welfare of the colony. The expedition thus inaugurated did not return until 1363 or 1364, and it has been suggested by such authorities as the late Dr Nansen and Professor Gustav Storm that its long absence may be explained by a visit to America, an extension of the original commission rendered likely by the circumstances already alluded to in connexion with the Greenland apostates. But while the year 1362 thus becomes an extremely probable date for the presence of a mixed Scandinavian force in America, the possibility has hitherto met with so little attention that no one would have been likely to hit on the idea without a great deal of close study of the subject, which would probably have resulted in the publication of his views. It is by no means the sort of thing which any smatterer with a casual knowledge of the fact of the Norse discovery would be likely to know; indeed, before 1887, when Storm alluded in passing to the possibility, the suggestion would have been quite original, and indicative of an unusual degree of research. The date, therefore, is one of the points which tells most strongly in favour of the authenticity of the inscription.

But, after all, a forger usually seeks to derive some personal benefit from his deception, even if it is only, as is apparently the case of the No Man's Land inscription, the pleasure of a quiet laugh at his victims. If once we are convinced by the evidence that the Kensington inscription cannot have been composed later than, say, 1864, the opponents of its genuineness are restricted to a very unlikely hypothesis. That a man would sit down in the middle of a dangerous wilderness, and spend at least two days in engraving an inscription of a length altogether

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superfluous for his purpose in a thicket where it was extremely unlikely ever to be discovered, and that he would then disappear, without a hint or a trace, surely violates common sense and all experience of human nature. Yet, granted the alibi established by the roots of the aspen tree, this is what is necessarily involved.

In these circumstances, the case against the stone must be said to rest almost entirely on its linguistic peculiarities. It has been hotly contended that many of the words, forms of spelling and grammatical usages, are conclusively inconsistent with a fourteenth century origin. It may at once be conceded that an extremely ignorant modern Scandinavian might have written something more or less on the same lines. For this reason, as has been indicated, the argument becomes double-edged when we are forced to ascribe to the author—if modern—a considerable degree of scholarship. And, if once we exclude the modern Scandinavian settler, the inscription does not in the least resemble what a smatterer of another race would have been likely to evolve. He would have been more likely to indulge in bad Icelandic. Admittedly, nevertheless, the inscription conflicts, in several respects, with the established *literary* practice of Sweden in the fourteenth century. Mr Holand, however, going carefully into the question word by word, is able to show precedent for practically everything. The principal grammatical peculiarity, the use of an uninflected singular form of the verb with a plural subject, is abundantly justified. So is the use of the 'th' rune for an unaspirated 'd'. For nearly every word, a fourteenth century parallel is actually produced, including 'ded' for 'död', which has been asserted to be English. Mr Holand, in fact, shows that many of his critics have been much too sweeping and hasty in their assumptions. 'Opdagelse' and 'har' still remain only partially justified, but there is force in the plea that the colloquial use of words usually antedates their appearance in formal literature. After Mr Holand's careful survey, it is difficult any longer to contend that the inscription is a linguistic impossibility. Yet, if this aspect of the question stood alone, I should personally be left with a slight bias against the authenticity of the record.

The most sensational evidence for the defence has, however, yet to be mentioned. Sensational, and therefore highly suspect, it will be said. I agree that it must be most carefully examined, and full weight given to every alternative, before it is accepted as conclusive. Let us proceed in this spirit. As a result of Mr Holand's known interest in the question, maintained over so many years, he has been the

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recipient of all sorts of things deemed by the finders or owners to have a possible bearing on the enquiry. Most of these, as one would expect, have turned out to be of Indian-French or English origin. But the residuum is decidedly impressive. First in importance come the heads of three battle-axes, recognized by authorities in Norway, Sweden and Denmark as typical Scandinavian weapons of the Middle Ages, and differing widely in form from the types in use in other countries or periods. There are also a medieval Norwegian hatchet-head to which the same considerations apply, and a spear-head and fire-steel which, though also of the appropriate form, are perhaps less strikingly distinctive. Putting aside for the moment the hypothesis that these are relics of the expedition commemorated on the Kensington stone, there seem at first sight to be two possible explanations. The first is that they were brought over, as family curios, by some of the settlers; the second, from which some critics do not shrink, that they have been deliberately imported and planted, to support the authenticity of the stone.

Let us consider both alternatives. Such things, in their country of origin, are exceedingly rare, even in museums. They are not the sort of thing of which the farmer immigrant would commonly be possessed, and, since they are things of no practical value, he would not burden himself with them, unless he regarded them with special pride. In the latter case, his treasures would certainly be brought to the attention of his neighbours. Again, they all show points of similarity indicating a contemporaneous origin in the same period, though they are not sufficiently alike in detail to appear the product of the same armourer. They can hardly all have belonged to one settler, since they come from widely separated districts. On the other hand, it is surely difficult to believe that several settlers should have happened independently to possess and to import rare curiosities so substantially similar. Moreover, the *provenance* is attested in each case, and, except on a theory of widespread and independent perjury, former ownership by a member of the present Scandinavian colony is shown to be impossible. Thus, Martha Davidson, widow, swears that one of the axe-heads was discovered by her late husband in 1894—before the appearance of the Kensington stone—a foot and a half below ground, under the root of a stump more than two feet in diameter. Another is traced to the possession of a pioneer settler, of Irish extraction, to whom it had been presented by Indians in the earliest days of the colonization of Minnesota. The spear-head was found in Wisconsin,

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in 1899, being ploughed out of a piece of hitherto unbroken ground. The fire-steel was brought up by a post-hole auger, from a depth of two feet, by the first settler in Polk county, Minnesota.

The suggestion of a deliberate planting is largely covered by the same objections. But it presents further difficulties of its own. It would not be a simple task to procure one medieval Swedish battle-axe ; to make so large a collection without being traced would appear nearly impossible. Moreover, the planter would presumably sow his evidence in the neighbourhood of the stone : he would not travel over most of Minnesota and Wisconsin, dropping now one, now another, of his *pièces justificatives*. If the suggestion is that he left them to be found as described, what a risk he ran that his labour would be wasted. If on the other hand he is supposed to have suborned the swearer of each affidavit, how lucky he must have been to have found so many persons in different places willing to lend themselves to his wicked, but rather motiveless, schemes, and never to have approached one who remained incorruptible and denounced the fraud ! I should in fact have ignored this explanation, had it not been quite explicitly put forward by a distinguished American historian, with whom I have corresponded on the subject.

The case is now stated in its main features. I will not venture on any dogmatic conclusion, my object having been to sum up judicially for the verdict of the reader. It appears to me, however, that this at least must be admitted, that Mr Holand has made out a case for a complete new hearing, and that objection to the stone can no longer be based on the fact that the weight of authoritative opinion has hitherto been adverse. Whether considered as an archaeological fraud or a unique contribution to historical knowledge, the Kensington Stone would seem to be a subject in which antiquaries may well take interest.

The Israelites in Sinai

by C. S. JARVIS, Governor of Sinai

IN my recently published book¹ I have advanced a theory as to the Wanderings of the Israelites and the loss of the Egyptian Host, which if not entirely original contains a considerable amount of fresh evidence based on ten years' experience of the Peninsula. Like all theories that tend to upset old beliefs this has resulted in a considerable amount of criticism and comment. The assumption briefly is that the Israelites never went to southern Sinai as has been firmly believed for the last 1900 years, but remained for the whole of the period of the Wanderings in the comparatively small cultivable area in the north-east corner of Sinai where they could grow corn for making bread and find grazing for their flocks. This part of my theory is by no means new as the same views have been expounded already by several students of the Wanderings, and I have merely corroborated their opinion and brought forward new evidence and arguments to prove the theory by virtue of my long residence in the Peninsula and knowledge of the country and climate. The explanation I have given of the loss of the Egyptian Host, however, is a novel one, and so far as I know has never been published previously though Sir William Willcocks² assumes that the disaster occurred in a lake in northern Sinai and not in the Red Sea; but the lake he selects is not the Bardawil as I maintain, and the incidents that caused the flooding were totally different from those I have suggested.

As the explanation of the mystery of the Wanderings has been dealt with very fully in *Blackwood's Magazine* for February 1931, and also in my book, it is intended to recount briefly the outline of the arguments that are advanced, and having done that to deal with the criticisms that have appeared in reviews and correspondence in the press. The greater part of these criticisms are extremely interesting as they emanate from students of the Bible who know their subject far better than I do, but who on the other hand have not had the advantage

¹ *Yesterday and To-Day in Sinai*, Blackwood, 1931.

² *From the Garden of Eden to the Crossing of the Jordan*, Spon, 1929.

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of knowing thoroughly the whole Peninsula or of studying its climatic and other conditions.

In the first place the only account of the Wanderings that exists is contained in the Bible, and the two books that deal with them are Exodus and Numbers. Unlike most epoch-making events in history there is no contemporary literature to enable us to read the other side of the story, and this rather tends to prove that the departure of the Israelites from Egypt and their subsequent wanderings in the desert were not of great historical importance to Egypt. Egyptian history is fairly complete considering the lapse of time, and there is no record of the Israelites fleeing from the country nor of the loss of a king and his army by drowning. All that we have to guide us therefore in forming an opinion as to what happened are these two books of the Bible, and tradition. Tradition is absolutely unreliable as no attempt was made to locate the various sites of the Wanderings till at least 1500 years after the event. One may presume that various devout pilgrims in the first century A.D. journeyed into Sinai in search of a great mountain, and having found several peaks in the south of the peninsula with the characteristics they were looking for made the rest of the Bible story fit in with their selection, and gave the old Biblical names of the halting-places to various wells and palm groves on the route. This route having been definitely selected, every Bible historian, who in recent days has made the journey to Sinai with possibly only a month or so at his disposal has been taken along this road and has been handicapped therefore in forming an unbiassed opinion. It should be borne in mind also that the present Mount Moses is not by any means the original selection, as at different times between the first and fourth centuries other peaks such as Gebel Serbal, Gebel Caterina, and Gebel Um Shomer have been accepted as the Mountain of the Law. I maintain therefore that the solution can be found in the books of Exodus and Numbers and nowhere else, and that the old tradition is absolutely valueless as evidence.

The reasons for thinking that the Israelites never went to southern Sinai are, briefly, as follows :—

(1) The Israelites were accompanied by a vast number of animals and grazing had to be found for these flocks. There is no grazing worth mentioning in southern Sinai as it is practically all solid or disintegrated granite. Moreover at all feasts and ceremonies during the Wanderings the Israelites ate bread, oil, meat and other foods, which proves that they must have been cultivating the soil. Nothing

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will grow in southern Sinai except a few olive and almond trees in certain of the valleys, but in northeast Sinai there is a large tract of country capable of producing excellent corn crops and providing grazing for large herds of animals.

(2) The Israelites shortly before the Giving of the Law, and also immediately afterwards, fed on quail that alighted round their camp in swarms. This phenomenon happens annually today in northern Sinai, but never in the south. The migrating quail land on the Mediterranean coast and nowhere else in Sinai. Three days after their departure from the Mount the Israelites fed on quail at Kibroth-Hataavah. Their rate of marching with flocks cannot have been more than ten miles a day, and this would definitely put the site of the Holy Mount about thirty miles south of the northern shore of Sinai.

(3) It is reasonable to suppose that the Israelites would avoid Egyptian authorities as much as possible, yet at the time of the Exodus the Egyptians maintained a garrison at Serabit el Khadim for the protection of their turquoise and copper mines, and Serabit el Khadim is only fifty miles from Mount Moses and fifteen miles from the route of the Wanderings. On the other hand in northern Sinai they appear to have maintained only frontier posts along the edge of the cultivation, which in those days was considerably further to the east than it is today.

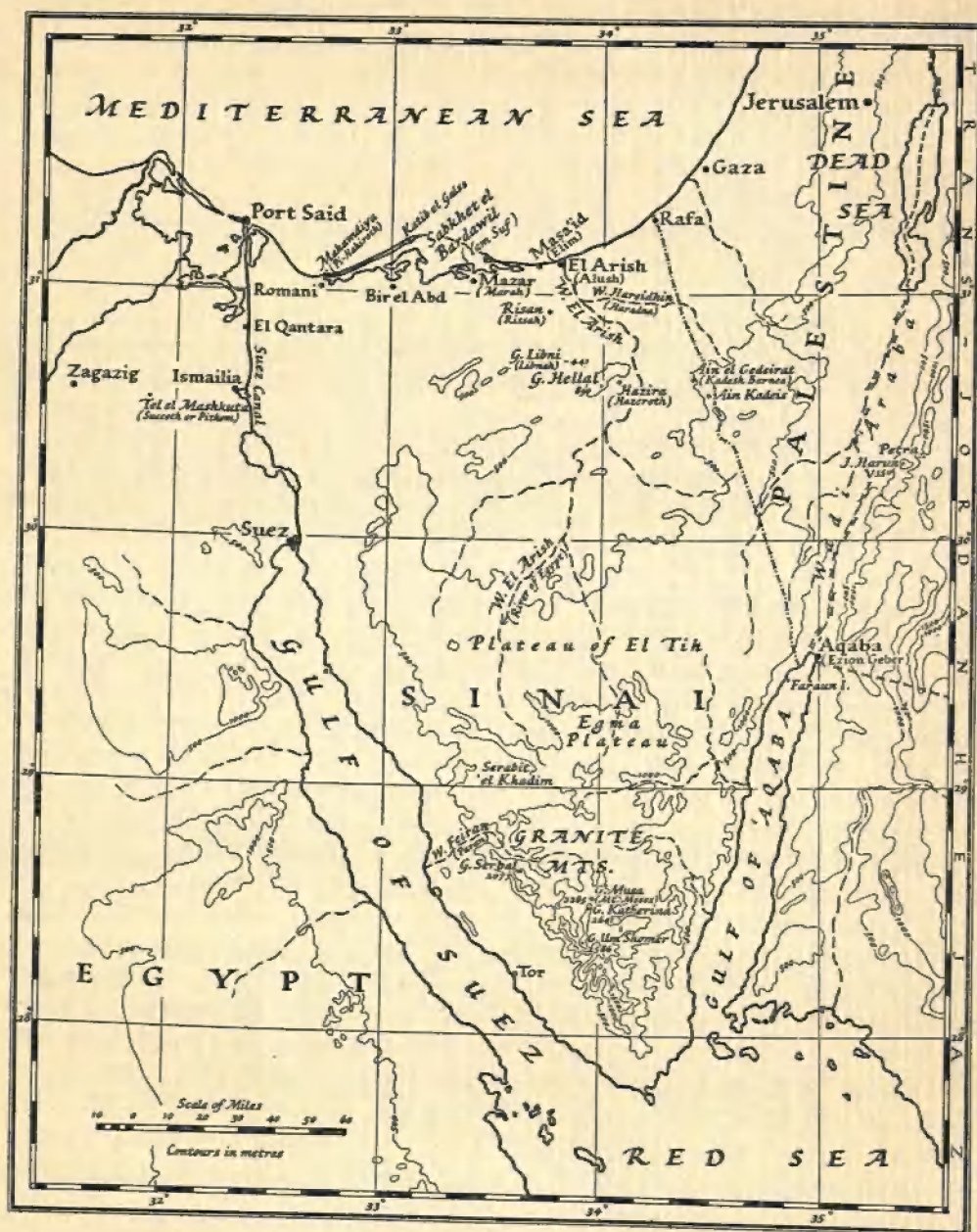
(4) The manna is said to be the deposit left by a small insect that feeds on the tamarisk tree. There would need to be a very large number of tamarisks to provide sufficient manna for the Host and in southern Sinai these trees only grow in isolated patches in the Wadis. There is evidence that there was a forest belt of these trees along the Mediterranean coast before the introduction of the camel practically denuded Sinai of all vegetation.

(5) In southern Sinai there is only one place with a name today that in any way resembles any of the halting-places mentioned in Numbers, and this is Feiran, which is supposed to be the Paran where the Amalekites were defeated. In the north there are several places with names that mean nothing in Arabic (in the desert every name has its meaning), and which are very similar to some of the names of the Wanderings. All these, such as Haradna, Rissah, Hazeroth, Libnah, etc., are in the cultivable area in the northeast of Sinai.

The only direct evidence against the theory is in Exodus, XIII, 17 and 18, as follows:—

‘And it came to pass, when Pharaoh had let the people go, that

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God led them not through the way of the land of the Philistines, although that was near ; for God said, Lest peradventure the people repent when they see war, and they return to Egypt :

‘ But God led the people about, through the way of the wilderness of the Red Sea : and the children of Israel went up harnessed out of the land of Egypt ’.

This certainly seems to point to the fact that they went south, but Red Sea is translated from the Hebrew ‘ Yam Suf ’, and ‘ Yam Suf ’ means Sea of Reeds and not Sea of Red. A sea of reeds rather suggests an inlet or lagoon with rushes and reeds growing thickly near the shores ; and Lake Bardawil on the northern coast of Sinai suggests itself not only as the Sea of Reeds mentioned but as being responsible for the loss of the Egyptian Host.

It has been pointed out³ that ‘ the words Sea of Reeds do not prove that the sea referred to is a sea of reeds any more than the English words applied to the same sea prove that it is a sea of red water ’. I submit that if a sea is called a Sea of Reeds that it must contain something to suggest reeds, rushes or, at any rate, sea-weed, and the southern shores of Lake Bardawil are covered with rushes. There are no rushes or reeds in the Red Sea, and in that particular arm of it—the Gulf of Suez, where the crossing is supposed to have taken place—it is singularly free from sea-weed of any kind. One never sees sea-weed floating on the surface of the water, neither is there a bank of it washed up on the shores after a gale. As to the contention that the name Red Sea does not prove it is a sea of red water I imagined that everyone was aware of the reason for the name, which is singularly apt. On either side of this sea there are high red mountains ; and on a calm evening when the sun is setting the whole of the Gulf is a dull crimson from the reflection.

The words ‘ led them not through the way of the land of the Philistines, although that was near . . . But God led the people about through the way of the Red Sea ’ does rather suggest a circuitous route to the south, or any direction but due east along the coast. There is, however, a more or less reasonable explanation. In the days of the Roman occupation of Egypt the recognized route to Palestine ran along the existing railway track by Romani, Bir el Abd, and Mazar. In Roman days it was a highway with halts every fifteen miles and was probably paved. It is reasonable to presume that it has always been the

³ Lt.-Col. G. F. Poynder in *Morning Post*, 5 October 1931.

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road to Palestine and was just as extensively used in 1500 B.C. as it was some 1600 years later. This road runs a matter of some fifteen miles south of the seashore and on the southern side of Lake Bardawil, and it is contended that Moses, to avoid any trouble with the posts at the watering stations, led his people along the actual shore, which was not 'the way of the Philistines', but was by 'the way of the wilderness of the Red Sea.' It is admitted that there is no great difference in the routes so far as distance is concerned, but it is submitted that if the southern route was a paved road with possibly Egyptian outposts on duty at the watering stations the other more roundabout track along the sea coast might be referred to as 'the way of the wilderness of the Red Sea'.

In the review of my book in *ANTIQUITY*⁴ the suggestion that the 'Pillar of Cloud and Fire' might have been the column of cumulus that forms on the Sinai coast during heavy weather, is criticized as being a not altogether suitable explanation of the cloud that led the Israelites on their march: the objection being that it would come in from the eastward and pass away over the Mediterranean to the west. This is what one would expect it to do, but actually this cloud-formation sometimes occurs two or three days before the weather breaks and remains stationary out at sea, increasing in size and depth of colour. When the gale springs up it moves southward over the land and breaks in violent but rather local squalls of rain and thunderstorms.

In looking for a suitable site for the Lawgiving, Gebel Hellal, some thirty miles south of El Arish, at once suggests itself. It is certainly not so impressive as Mount Moses, but it rises up sheer from an alluvial plain and being 890 metres high is very imposing when one takes the surrounding country into consideration. Its name 'Hellal' is particularly apt as Hellal in Arabic means 'lawful', and the Arabs of Sinai, who have an explanation for most place-names, cannot account for this particular mountain being called 'lawful'. Some critics have suggested that the name is really 'Hilal', which means 'crescent', but this is wrong—the name of the mountain is definitely 'Hellal'. It was also explained that it does not suggest a great and smoking mountain which can only have been caused by volcanic action. This is an interesting point to raise as I find that a large number of people believe that the various fiery manifestations that occurred during the Wanderings were caused by the volcanic nature of southern Sinai.

⁴ March, 1932, p. 116.

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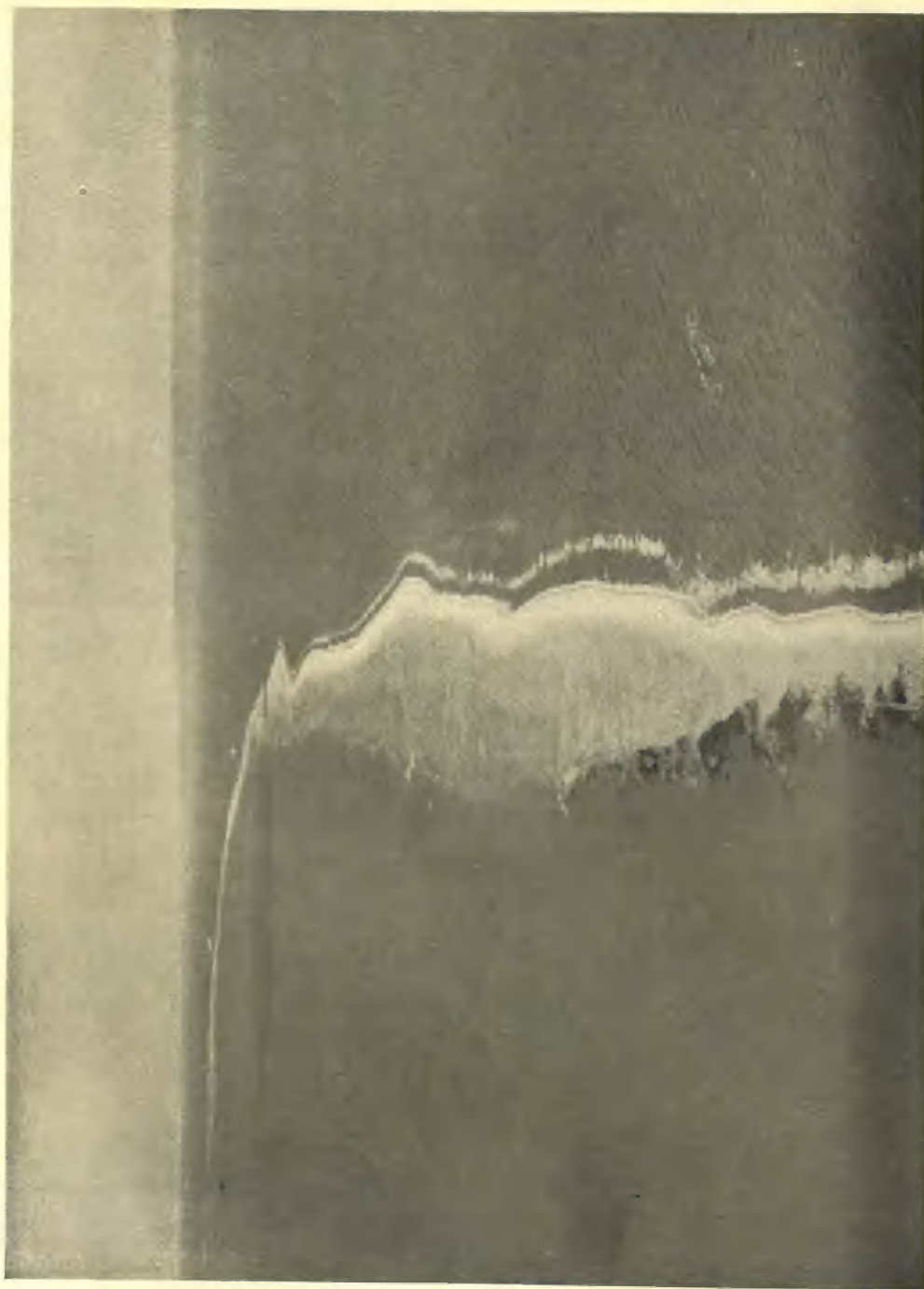
This is an apparently feasible explanation of the 'burning bush', 'pillar of fire', and 'Mt. Sinai altogether of a smoke', and the assumption appears reasonable when one realizes that southern Sinai is tossed and tumbled at every angle by volcanic action. Unfortunately for this belief the Survey Department of Egypt, who have most carefully explored the Peninsula from a geological as well as a geographical point of view, give it as their considered opinion that Sinai has not been actively volcanic for at least a million years, and certainly not during the human period. This effectually dispels the theory of attributing the fiery manifestations to volcanic action, but even if the views of the Survey Department are not accepted one must bear in mind that Gebel Hellal is also volcanic and was thrown up at the same time as the rest of Sinai.

I am of opinion that the mention of smoke, fire and quaking was inspired by the violent thunderstorms to which Sinai is subject. The Israelites coming from the Nile Valley, where thunderstorms are very rare and where the land is absolutely flat and featureless, were no doubt overawed and terrified when they saw a storm raging round the peaks of Gebel Hellal. When there is bad weather in Sinai the top of this mountain is always wreathed in smoke-like clouds, which are rent from time to time by forked lightning, and the terrific crashes of thunder actually cause the earth to quake. A storm of this description bursting over a rocky and lofty peak must have been a weird and terrifying sight to a people who had never seen previously either a thunderstorm or a hill more than twenty feet high.

Beyond the fact that Gebel Hellal possesses a name that lends itself to the theory that it is the Mountain of the Law, and is moreover the highest peak in the area where the Israelites probably sojourned for the greater part of the Wanderings, there is no further evidence to prove that it is actually the Mount, and it is just as possible that the Law-giving occurred on any of the six or seven imposing massifs in that locality. There are many historians who hold the view that the actual event took place in the Petra area and that Mount Haroun is the site. The argument against this theory is that only three days after their departure from the Holy Mount the flight of quails descended upon the camp at Kibroth-Hataavah, and this must have occurred on the Mediterranean coast, for the very simple reason that nowhere else are the quails seen in Sinai.

I have had my attention called to a book on Egypt⁵ said to have

⁵ *The Chronicles of Osiris.*



AIR-PHOTOGRAPH SHOWING THE NARROW SAND TRACK BETWEEN THE MEDITERRANEAN SEA AND LAKE BARDAWIL

One of the cuts made by the sea is shown in the distance ; the dark patch to the left of it is the sea-water flowing in

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been written by clair-audient dictation by an ex-officer of the Royal Air Force, who has never been to Egypt and moreover knows nothing of Egyptian history. The book is remarkable as it shows an intimate knowledge of the ancient history of that country which is very difficult to account for when one considers that it comes from the pen of a man who was absolutely uninformed on the subject. Incidentally the book upsets a large number of theories about Egypt's past that have been accepted by Egyptologists. The flight of the Israelites is referred to as follows :—

' Osi-ra-es (Moses) had organized the tribes after this manner : the Ark of Amen in charge of Asar-haron proceeded to lead the tribes and behind it came all the elders and chief priests of the temple, which was at that time only a portable building. Next came the women and children and all the civil population, and bringing up the rear the men of war who guarded the exodus of the people. The frontier guards having heard what was taking place shut themselves up in their strongholds or joined with the migrating tribes. Then was it that a terrible storm, in the midst of which the heaviest fighting took place, broke over the whole province of the Delta. The Egyptians were very persistent and followed close upon the heels of the retiring tribes, till at length the marshlands at that time existing round the Sinai Peninsula were reached. The tribesmen under the command of Osi-ra-es retreated behind these marshlands drawing the Egyptians after them. When at length both armies were between the peninsula and the marshes, the forces of Osi-ra-es turned and offered fight to the Egyptians, who were so completely taken by surprise that they fled in panic, many of them being submerged in the marshes in their hasty flight ; and among those who perished in this manner was the king's eldest son and heir, Thothmosis. Thus you will see how the tribes referred to in the Bible as the Children of Israel crossed over into the desert of Sinai on their way to Palestine '.

This is very much the same as the theory that I advance except that my view is that the sea broke through into the marshlands and completed the destruction of the host.

A very convincing touch is the sentence that deals with the frontier guards shutting themselves up in their forts or joining in with the migrating tribes. In the many articles written about the Wanderings it has never occurred to anyone to wonder what action was taken by these men on out-post duty on the frontier, and yet we know there must have been frontier guards for their forts, as sand and clay mounds,

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exist along the flat lands to the east of Kantara and Ismailieh. Egypt in those days was constantly in a state of war with her eastern neighbours, and it is obvious she must have maintained a strong force on the edge of the cultivation to hold back the first shock of an invading army, and in normal times to guard the settled part of the Nile Valley from Beduin raids. The word Beduin may have a modern sound, but there is no reason to suppose that a harsh wilderness like Sinai was ever inhabited by any other race. It matters little whether they are called Anus, Mentus, Ishmaelites, or Saracens—the conditions are such that none but a race similar in every respect to the Beduin could exist in Sinai, and the Beduin of today and the Anu of 5000 years ago are in all probability practically the same. With the exception of his rifle, which he has acquired from Europeans, there is nothing about the present-day Beduin, his mode of life, his dwelling and his implements that has changed in the course of time.

When the frontier guards were faced by a resolute and desperate host of fighting men that completely outnumbered them they in all probability did exactly what the writer of *Chronicles of Osiris* states: they shut themselves up in their strongholds and took no action, nor joined in with the migrating tribes.

My theory of the loss of the Egyptian army is that the Israelites were moving eastwards along the actual coastline, and when one considers they were accompanied by their women and children and their flocks of sheep and goats it is obvious they were not travelling very fast, so that a well-equipped force setting out from the vicinity of say Zagazig would have no difficulty in overtaking them. Along the coast line some thirty miles from Port Said stretches the huge Bardawil lake that normally is a flat clay pan separated from the sea by a strip of sand from one to three hundred yards in breadth. In heavy weather the sea breaches this strip of sand and fills up the whole depression to a depth of from three to six feet. In a few days these cuts silt up again and the water dries out in course of time, so that Lake Bardawil is normally dry with a hard clay surface that will support a man on foot, but will most definitely not carry a horse or wheeled vehicle. The pan is thirteen miles at its widest, but in parts it is no more than three or four miles from its southern shore to the strip of sand that divides it from the Mediterranean.

The Egyptian army were travelling along the inland road that runs from Kantara through Bir El Abd to El Arish, and their scouts brought in information that the Israelites were on the coastal track.

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Possibly they could see them, and the obvious thing to do if one did not know the country was to cut across the clay pan and head them off. This they did and by the time they had travelled a mile or so and had reached the softer parts of the pan they were in serious trouble—the horses wallowing to their girths and the wheels of the chariots breaking through the crust to the soft mud underneath to the hubs of the wheels. One has only to read carefully Exodus, xiv, 24–28 to see that the Egyptians were in the first place bogged, and that the swallowing up of the army by the sea did not occur till the following day. There is absolutely nothing in the actual text to support the old theory of our childhood days, to which so many people cling, that the sea opened up to allow the Israelites to pass and then rolled back in a huge wave over the Egyptians, and it is difficult to understand how the belief ever originated. The story as it is told suggests an army floundering in the mud and in serious difficulties long before there is any mention of the sea. 'Entangled in the land' obviously means 'floundering in the mud', and this is proved still more definitely by the words 'and took off their chariot wheels, that they drove them heavily'.

There was probably a very strong gale working up and in verse 20 a cloud is described as white and shining on the camp of the Israelites and dark on that of the Egyptians. This is a good description of the violent cloud-bursts that occur in Sinai every winter and spring. They are very local, and outside their area the sun shining on the falling rain makes it appear as a shimmering silver curtain, but inside the cloud-burst it is nearly as dark as night. The heavy rain added to the difficulties of the already bogged Egyptians, and then the violent squall brought up a high sea, the narrow spit was breached, and the sea came rolling in on to the pan—as it does at the present time whenever there is heavy weather. It does not need any vivid stretch of the imagination to visualize the disaster, as it is an occurrence that could happen today, and moreover it is not necessary to read the verses of Exodus in any other way than in their literal meaning. Moses' song of triumph in chapter xv was a poetic rendering of the epic story, and cannot be regarded as such reliable evidence as the bald facts related in the previous chapter.

The statement that the Israelites walked upon the dry land in the midst of the sea with the sea as a wall unto them on their right hand and on their left is precisely the feeling that one gets today if one walks along the narrow sand spit with the Mediterranean to the north and the Bardawil lake to the south, and no land in sight. One can easily

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understand an ignorant and fanatical people would imagine that the pathway had been opened up for them by Divine power.

Although critics of my book have disputed most of the theories I advance for placing the Wanderings in the north of Sinai and not the south, so far no one has found any weak spot in the Bardawil theory except one sweeping and uncorroborated assertion that 'any unbiassed reader of Exodus cannot fail to see that the conditions of the site do not in any way coincide with the story as told'. The loss of the Egyptians is referred to in later books of the Bible, but these cannot be regarded as first-hand evidence nor used to refute in any essential details the story as told in Exodus, which is not an account of an army suddenly swallowed up by a tidal wave or by the rolling back of waters by Divine power. It is most definitely a story of an army bogged in soft mud for the best part of a day and a whole night, and engulfed the following morning by an influx of the sea.

References as to the Mahommedan belief that the loss of the Egyptian host took place in the Gulf of Akaba have been asked for,⁶ but I regret that I cannot supply any. So far as I can make out it is merely an Arab tradition, and as my knowledge of Arabic literature is very limited I cannot say if there is any written record. In any case it would not be earlier than about A.D. 600. The Beduin have some vague idea of the doings of all the patriarchs and prophets, and are particularly interested in Moses because of his ability to strike rocks and find water, which in their eyes is the most useful gift a man can possess, and also in the story of Noah and his Ark. They say that Moses was in Sinai with the 'Yahudi' (Jews) and that Pharaoh's host was lost in the Gulf of Akaba in the vicinity of Faroan Island—Faroan being the Arabic form of Pharaoh.

In a short article it is impossible to deal with every point and all the evidence in detail, but in these notes I have endeavoured to answer the arguments that students of the Wanderings have advanced against this new theory.

⁶ ANTIQUITY, March 1932, p. 117.

Maze Symbolism and the Trojan Game

by W. F. J. KNIGHT

IN his detailed account of the chambered cairn of Bryn Celli Ddu,¹ Mr W. J. Hemp describes a remarkable pattern stone, found on the site lying flat in a floor of purple clay, as it had been deliberately placed by the builders of the monument.² Mr Hemp says that 'The recumbent position of the stone . . . and the disposition of the pattern



DIAGRAMMATIC DRAWING OF PATTERN STONE, BRYN CELLI DDU, SHOWING CONTINUITY OF DESIGN
By permission of the Society of Antiquaries of London and Mr W. J. Hemp

inevitably suggest that it was intended to be set upright in the ground at some stage in the funeral rites in such a way as to display the pattern'. He adds that the meaning of the pattern is unknown but that some form of magic is perhaps the most obvious explanation, and cites references for the occurrence of the style in megalithic monuments of Brittany³

¹ *Archaeologia*, 1930, LXXX, 179-214.

² *Ibid.* 197f. and pl. XLIX, figs. 1 and 3.

³ M. et E. Péquart et Z. le Rouzic, *Corpus des Signes gravées des Monuments mégalithiques du Morbihan* (Paris, 1927). Cf. especially pls. 1, 3, 5, 14, 19, 23, 31, 33, 35, 50, 59, 71, 72, 73, 75 (a good example), 80ff. (where wheels are suggestively shown: cf. *infr.*), 96, 99ff.

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and Ireland.⁴ The pattern is incised on the stone, with a leaf-shaped outline. The lines are zigzag, and the design at once suggests an inaccurately rendered maze formation.

This motive, or rather the cognate motive of the spiral, occurs again in the monument. It is formed on one of the upright stones⁵ as a small and simple design. But there is something else much stranger than that. Of the plan of the structure itself Mr Hemp⁶ writes :

‘ Further study of the design brought to light a very surprising fact. To a spectator standing without the outer passage the monument appears to have a perfectly symmetrical arrangement of four converging walls, all terminated at the portal by upright stones . . . Analysis of the plan, however, reveals a very different position. . .

‘ This end of the inner circle therefore is continuous with the north wall of the passage. This wall runs on without a break, curving round to form the chamber, and returning as the south wall of the passage, to complete the circuit of the monument . . . the line of walling therefore runs without a break twice round the monument. . . In fact the two “ circles ” may be considered as together forming a gigantic spiral, in which is a loop comprising the passage and chamber.

‘ If then, the whole inner area of the monument enclosed by the ditch is a “ holy place ”, the burial chamber is in it but not of it, being completely shut out by the “ loop ” in the spiral ’.

In other words, the spiral form occurs three times in the monument, once as the actual structural design. I first connect these three occurrences together, and propose to find a similar interpretation of all three. I assume that the best explanation is some kind of magic, and approach the attempt to say what kind. Next I ask what are the associations of this motive in other places and times ; for though megalithic man has not articulately communicated his secrets, the pattern in its variation is so extremely general that its meanings may be guessed to be on the whole coherent, if not quite the same, always.

The maze form—which is an elaborated spiral—gives a long and indirect path from the outside of an area to the inside, at a point called the nucleus, generally near the centre. Its principle seems to be the provision of a difficult but possible access to some important point. Two ideas are involved : the idea of defence and exclusion, and the idea of the penetration, on correct terms, of this defence.

⁴ G. Coffey, *New Grange* (Dublin, 1912) ; E. A. Cornwell, *Procs. Royal Irish Academy*, xvi, 72 ; W. Frazer, *Procs. Soc. Ant. Scotland*, xxvii, 294.

⁵ *Archaeologia*, 1930, LXXX, 184, pl. XLV, fig. 1.

⁶ *Ibid.* 200f. ; cf. pls. XLVI, fig. 2 and LIV, fig. 1.

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I argue that these conceptions, to our minds necessarily implied by the maze form, are inherent also in early presentations of it, and were partly, at least, intended by most of the artists who employed the principle in early times, though the intention need not have been explicitly remembered and understood. In fact at Bryn Celli Ddu there is inaccuracy of treatment, as though the artists were attempting to follow a tradition that they could not clearly understand; a thing not uncommon in early monuments.

Maze rituals can sometimes be identified in the long history of the maze form. They have been connected with solar ceremonies. This explains some circular movements, perhaps, but not the principle of obstructed penetration. I suggest that the obstructed penetration is the concept nearest to the base of many of the rituals and forms, though I do not deny the possibility of solar relevance altogether. This concept can be applied in its positive or negative denotations in much of the material which offers any chance of interpretation. The Egyptian hieroglyphic which is an element from which the maeander pattern can be derived stands for 'house'. The Cretan symbolism of maze and Minotaur, which seems a pure form in which the correlatives of exclusion and entry both appear, is expressed on coins of Knossos in the maeander form, which has been thought also to represent a house or house wall on Attic vase paintings. In a bold attempt to interpret the Cretan scripts it has been suggested that the letter M is a derivative of the symbol of the template by which maeanders were made, and that accordingly the same letter was used as a determinative for 'sanctity'.⁷

That the myth of Theseus and the Minotaur expresses something which was also expressed by ritual is generally believed. Ariadne's crane dance⁸ is admitted to represent one of these rituals. So is the Troia, Truia, or Trojan game, a ritual ride of armed horsemen practised in early Italy and much encouraged by the Roman emperors.

Now if the Troia is a maze ritual, it seems that a chance of arguing from it to the general meaning of the maze form, if it had one, has been missed. Classical scholars have investigated the Troia, and anthropologists and folklorists have considered the maze patterns found in material representations; but the combination of results is unsatisfactory. The Troia is certainly old in Italy⁹ and probably enough

⁷ F. Melian Stawell, *A Clue to the Cretan Scripts* (London, 1931), 40-44.

⁸ *Iliad*, XVIII, 590ff.; Pollux, IV, 101; Tryphiodorus, 352ff. (where the scene at the entry of the wooden horse into Troy is compared with the crane dance. Cf. *infr.*).

⁹ Suetonius, *Augustus*, XLIII, 2; Dio Cassius, XLIII, 23.

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it was also at home in Troy. Mazes are very generally called by the name of 'Troy'.¹⁰ 'Troys' that existed in ancient Italy,¹¹ as they exist still in North Europe, were surely places where there were mazes: Pliny mentions the treading of outdoor mazes as a game.¹² Probably the presence of these 'Troys' helped the spread of legends of Trojan foundations, though some of the legends seem to have been partly true in a more literal sense. At any rate, the Troia certainly followed a maze form for its convolutions. In a very usual fashion, Vergil¹³ betrays the truth by saying that the movements of the Trojan game were *like* the intricacies of the labyrinth at Knossos; it is in this way that he divulged the secret of the Golden Bough by saying that it was 'like' what in fact it actually was in the beginning—the mistletoe. Folklore misunderstood and forgotten frequently becomes poetic metaphor in the end; a truth which psychologists can partly explain, and which other investigators can often use with advantage.

Vergil is confirmed by the famous Tragliatella Oenochoe, Etruscan work of the sixth century.¹⁴ On that are shown armed horsemen riding; they trail behind them a large incongruous maze, which quite obviously expresses the path which they have taken; and to the picture is added the word 'Truia'. In early Italy, therefore, armed horsemen used to perform 'musical rides' (with or without music, probably with), in which they made maze movements, clearly enacting something symbolically.

The meaning of the Troia is the harder to understand because it seems to have been a secret in antiquity. There was a book about children's games by Suetonius, however, and Servius¹⁵ says that in it he identified the Troia with 'Pyrrhic' dances. The Troia was called a *ἱερὰ ἱπποδρομία*,¹⁶ which means that the horses were important to its nature, and also that it was connected with some supernatural relevance. *ἱερός*, which is not the same as *ἄγιος*, or *ὁσῖος*, does

¹⁰ W. H. Matthews, *Mazes and Labyrinths* (London, 1922), 4, 23, 52, 71, 81, 88, 89, 91, 93, 94, 98, 129, 151, 156, 162, 181, 197, 202, 211, 216, 225, 233. For material about mazes I confine references to this excellent and accessible book, which has many good illustrations and a good bibliography. In future I refer to it as M.

¹¹ Livy, I, 1; Verg. *Aen.* I, 48; Dionys. Hal. *Ant. Rom.* I, 53.

¹² *N.H.* xxxvi, 85. The whole passage (84ff.) is important. Pliny seems to consider a multicursal form, giving the possibility of mistake, original (84).

¹³ *Aen.* v, 588ff.

¹⁴ M, 157 (after Deecke). It is countlessly reproduced.

¹⁵ Servius *ad Verg. Aen.* v, 602.

¹⁶ Plutarch, *Cato minor*, III.

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not mean 'holy', but something which would be expressed today by reference to the possession of *mana*, good or evil magical power. The use of the word for the Troia has been thought difficult to explain,¹⁷ and I do not know any published explanation of it. This is not all. The Troia is also called a 'mystery' in the ancient sense:¹⁸ that is, it was a ritual associated with the exclusion of the uninitiated. This also has not been explained.

Fortunately however there are certain hints. Elsewhere not the Troia, but something which was very clearly a maze ritual, is associated with 'mysteries'. Apparently at the Eleusinian Mysteries the initiate had to pick his way through something rather like a maze.¹⁹ This circumstance has been compared by Mr Colin Still²⁰ with Shakespeare's reference to 'forthrights and maeanders', through which in *The Tempest* Alonso's party has to go to reach its allegorical destination.²¹ Both *The Tempest* and the Eleusinian Mysteries, besides much else in art and religion, are shown by Mr Still to be manifestations of what he calls the Universal Myth. The Universal Myth is an expression of the conditions of the soul's journey to perfection; prophetic thinkers and poets present their allegories of human life in remarkably similar symbols. The maze and the 'forthrights and maeanders' certainly mean the doubt and sorrow through which men pass on their way to revelations of religious truth. The end of it all is again poetic figures. Blake says that 'sorrow is an endless maze' and Francis Thompson calls thought a labyrinth, until he found the truth of God. Vergil²² himself sets at the beginning of the Sixth *Aeneid* a skilful representation of the Cretan Labyrinth, pictured on the gates of the Cumaean temple, and clearly meant to symbolize²³ the stage of doubt and confusion through which, as through a gate, many must go, who, like Aeneas, are to reach the Divine truth. The dark wanderings which were an early part of the mysteries correspond well with the 'blind footsteps' of which Vergil writes.

The idea of initiation then agrees with the suggestion that the maze form in general means exclusion and, correlatively, entry on

¹⁷ F. Rasch, *De Ludo Troiae* (Progr. Jena, 1882), 7.

¹⁸ Galenus, *ad Pis.* 93of. (ed. D. C. G. Kühn [Leipzig, 1827, vol. xiv, 212]). Cf. A. von Premerstein in *Festschrift für Benndorf* (Wien, 1898), 261.

¹⁹ Colin Still, *Shakespeare's Mystery Play: a study of The Tempest* (London, 1921), 132f., citing Plato, *Phaedo*, 108A; Lucian, *Cataplus*, xxii.

²⁰ Still, *ibid.*

²¹ *Ibid.* 27.

²² *Aen.* vi, 24ff.

²³ W. F. J. Knight in *Classical Review*, 1929, XLIII, 212-13.

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certain conditions. The Eleusinian Mysteries and the Sixth *Aeneid* give the idea in an advanced stage, not unlike the stage represented by the medieval church mazes.²⁴ They, again, are not certainly explained and there is disagreement.²⁵ These mazes certainly seem to have been used for a kind of pilgrimage; the tracing or treading of a maze was probably imposed as a penance, and the nucleus is sometimes called Heaven or Jerusalem.²⁶ One maze is said to represent the world, and the track through it 'the natural man's path to spiritual life'.²⁷ Apparently ideas of exclusion and entry belonging to the maze were adopted and used by the Church: for the maze seems to have been brought within ecclesiastical sanction from a secular, perhaps even a pagan, existence outside.

A maze can be a symbol of secrecy.²⁸ This part of its content is found in the application of the maze motive to the story of Rosamund, an illicit love of Henry II of England. He built her 'Rosamund's Bower' at Woodstock in the form of a maze, and visited her there.²⁹ In this story scandal and folklore seem to enjoy equal places: though what the truth of each is it is hard to say.³⁰

²⁴ M, 54ff.

²⁵ M, 66ff, where possible uses are discussed.

²⁶ M, 60, 64, 66, 68.

²⁷ M, 57; cf. 68.

²⁸ M, 53, 96, 194; cf. Knight, *ibid.*

²⁹ M, 164ff.

³⁰ The maze symbolism seems somehow to be associated with maidenhood. Cf. M, 150, for the name 'Nun's Fence'. A vignette on the Tragliatella oenochoe seems to require this explanation: if it has relevance to the magic walls of the Truia, it may represent the correlatively opposite magic. In North European and other mythology the overcoming of difficulties by a hero, often with the help of a magical horse (v. *infr.*), frequently precedes union with some hidden princess. Cf. G. W. Cox, *The Mythology of the Aryan Nations* (London, 1870), I, 115, 151ff., especially 154; L. Laistner, *Das Rätsel der Sphinx* (Berlin, 1889), II, 51, 117f., 143ff.; A. de Gubernatis, *Zoological Mythology* (London, 1872), 293ff., 297; E. Jones, *On the Nightmare* (London, 1931), 269ff. Cf. E. Hommel in *Orientalistische Literaturzeitung*, 1919, xxvi, 63ff., who from Babylonian and Cretan evidence connects the spiral labyrinth with human anatomy and with the underworld, the one being the microcosm of the other.

Mr W. J. Hemp tells me that in his opinion the suggestion made by Mr T. Cyriax in the *Archaeological Journal*, 1921, LXXVIII, 205-15, that there is reason to suppose anatomical and genetic symbolism in the form of some barrows, is strongly supported by the plan and certain details of Bryn Celli Ddu. At the same time he considers that the plan represents a step in a long process of development, and that many of its features cannot be associated with any such symbolism. Cf. especially Cyriax (*ibid.* 211): 'To enter the next world, therefore, the spirit would have to be *re-born*' . . . 'The object of the tomb-builder would have been to make the tomb as much like the body of a mother as he was able'. The article is important and seems convincing.

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In describing the performance of the Troia in Sicily before the Trojans reached Italy, Vergil³¹ apparently says that Iulus introduced it 'when he was surrounding Alba Longa with walls'. Servius³² seems to have thought that there was a difficulty here; as if Vergil might be supposed to mean that the Troia originated in Italy, though of course it had previously been performed in Sicily. He therefore says that Vergil means that it was merely revived at Alba. This does not matter very much, but it seems to be an instance of one kind of difficulty which betrays a secret. Vergil usually means something definite by his words, and if he means what he says here, and is right, the maze movements of the Troia were at first part of a foundation ceremony. If so, the idea of exclusion is very appropriately represented.

At this point it is worth while to mention the Salian dance. At certain times the Salii danced round Rome, armed, but carrying weapons more like large drumsticks than anything else, with which they beat their shields.³³ Their dance has been called a 'Troia on foot'.³⁴ This is interesting, because the Salii take part in a procession round the city,³⁵ an 'amburbium' to preserve it from harm in a time of danger; that is, a function very like the function here suggested for the Troia. It has been thought that the 'tribuni celerum', the commanders of the Roman cavalry, must be present at the Salian rite.³⁶ It is recorded that both Salian dance³⁷ and Troia³⁸ were introduced into Italy from

³¹ *Aen.* v, 596ff.

³² *ad. Verg. Aen.* v, 598: 'rettulit: innouavit quod ante iam fecerat'.

³³ J. E. Harrison, *Themis*² (Cambridge, 1922), 194ff. Part of the function of the Salii, who are comparable to the Curetes, may have been in some sense to frighten away evil spirits with their noise. On the Tragliatella oenochoe dismounted performers may have long drumsticks. On the place of the Salii in ancient circle magic and their connexion with the Troia cf. S. Eitrem, *Opferitus und Voropfer der Griechen und Römer* (Christiania, 1915), 27f.

³⁴ A. von Premerstein in *Festschrift für Benndorf*, 265.

³⁵ Lucan, *Phars.* I, 592-606. Canon Stacy Waddy has suggested that in the Hebrew Psalms, interpreted as liturgies of service, the mention of the shield usually implies a ritual 'march round' some sacred person or object: cf. *Psalms*, v, 12, xviii, 30f., xxxiii, 20, lxxxiv, 9. Wheels seem to be defensive symbols sometimes; cf. W. Simpson, *The Buddhist Praying Wheel* (London and New York, 1896), 116, 258, etc., and *supr.*, note 3.

³⁶ E. Meyer, *Kleine Schriften*, 1924, II, 283, citing *C.I.L.* I, pt. 1², 234 (the *fasti Praenestini*; accepting the view of von Premerstein, 264, which had been criticized by J. Toutain in Daremberg et Saglio, *Dict. des Ant.* v, 493-97, s.v. Troia.

³⁷ Festus, 329A.

³⁸ Verg. *Aen.* v, 545ff., etc.

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lands to the east of it. The name of the inventor of the Salian dance is interesting. It is reported to have been Saon,³⁹ the 'saviour' or 'protector'.

It appears then that both the Salian dance and the Troia were meant by their intricate armed movements to strengthen the defence of a city by supernatural means. They emphasized in fact the effective distinction, so important in Italy, between the powers of without and within.⁴⁰ The Troia is certainly a kind of 'lustratio': the word could be employed for magical movements round a defended place. The action must have been thought to create an additional and abstract wall of power to exclude hostile influence.

This now shows why the Troia could be called *ἱερός* and a 'mystery'. It also shows why one of the many strange names given to mazes is justified. They are called 'walls' in many places. In England there are at least four instances of the name 'walls of Troy',⁴¹ and in Finland for example a maze is called 'Giant's Fence'.⁴²

This aspect of the maze form has been overlooked. That is, investigators have emphasized the peripheral movements of ritual, and have noticed the penetrative movement from the outside of a maze to the inside, without sufficiently regarding the quite correlative meaning of 'exclusion', contrived by the peripheral movements, and conditioning the penetration. A maze is in fact a wall of great magical strength with an entrance designed to be difficult for the unauthorized.

The name 'wall' therefore belongs to mazes significantly. Another name, 'Troy', very frequently applied to them, has been more adequately discussed. The theory that this name was given in literary reminiscence of the classical connexion of the Troia with the Homeric Troy is in conflict with the facts, especially with the wide distribution of the name and its apparent association with the older folklore of areas

³⁹ O. Gruppe, *Griechische Mythologie und Religionsgeschichte* (München, 1906), I, 199, note 11; Salius danced 'in honour of Alesus (ibid. note 12: cf. Athena "Alea", which probably means "defender") son of Poseidon'—who is of course guardian of city walls. Cf. ibid. 225. D. Servius (*ad Verg. Aen.* II, 325), says that the Salii were at first called Sai, when they looked after Penates in Samothrace. This function—they were *penatium antistites*—betrays defensive connotations.

⁴⁰ Cf. W. Warde Fowler in *Anthropology and the Classics* (Oxford, 1908), 174, etc. Cf. Eitrem, 6, 18, 29ff., on the question whether the original purpose of these rites was cathartic, as he thinks, or apotropaic, according to Eitrem a developed function.

⁴¹ M, 73, 78, 87, 92: cf. 201.

⁴² M, 150f. In North Europe 'stone fence', 'nun's fence', 'round castle', 'giant's castle' are found: there is also a 'stone dance' connected with mazes.

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outside the Roman occupation.⁴³ It is unsafe to attribute the introduction of the maze and its ritual practices—thought to be connected with Morris dancing—to Roman influence. This derivation is unlikely in Britain and scarcely possible in North Europe, though the influence of the Romans, who have left careful representation of mazes, sometimes with a minotaur—or centaur—in the nucleus, at many places, should not be excluded altogether.

Literary equations, then, will hardly explain the application of the name 'Troy'. It is more likely to express some description of mazes or maze movements. This suggestion has been made before. It has been proposed⁴⁴ more than once that the name of a maze in Wales, Caer Droia, ought to be derived from a root meaning 'turn'. In this context it is not always remembered that in 1840 Klausen⁴⁵ suggested that the Italian Troia should be connected with a Latin verb 'trouare', which is said to denote some kind of active movement. It seems scarcely possible to decide the exact early meaning of the maze name 'Troy'; but the facts seem to show that it is a name which ultimately belongs to mazes, not a literary reminiscence of the city of Troy from which Roman writers derive their Trojan game.

This solves one difficulty, but makes others. If 'Troy' is an original name for mazes in North Europe, it is satisfactory that a similar suggestion for Italy has been made. On the other hand, other names from the Trojan cycle of legend are also applied to mazes, especially the name 'Iulus'⁴⁶ in different forms. This is not a great

⁴³ M, passim, especially 147ff.

⁴⁴ M, 92, attributing the suggestion to W. H. Mounsey, who made it in 1858. I owe the following information to the kindness of Professor J. E. Lloyd. In old Welsh literature the city of Troy is sometimes styled 'Caer Droia'; and the same name is also applied, for reasons difficult to apprehend, to a maze or labyrinth. On the second point Professor Lloyd refers to a notice, with plan, furnished by Peter Roberts, *Cambrian Popular Antiquities* (London, 1815), 212f.; who also suggests that the real connexion of the name is not with Homeric Troy, but with the word 'tro', 'turning'. Mr W. J. Hemp kindly contributes the following note:—"The ordinary Welsh word for "to turn" is "troi". The best dictionary (Anwyl) reads as follows:—"Troï: to turn, to revolve, to stir, to convert, to become, to plough. There is also:—tro -ion -iau; turn, curve, screw, twist, time, occasion, walk, tour, conversion. Troad; bend, turning, flexion".

⁴⁵ R. H. Klausen, *Aeneas und die Penaten* (Hamburg, 1839-1840) II, 82off. Rasch (*De Ludo Troiae* [*Progr. Jena*, 1882], 7), commenting on this, suggests a root meaning 'turn'. Toutain (*Dar. et Sagl. s.v. TROIA*) accepts some such origin for the name.

⁴⁶ M, 71, 78, 90, 173, 230.

difficulty, because there is no reason why the original name of Troy should not have attracted other names by a literary application. What will not account for all the facts may account for some. Another difficulty is more interesting. If the Trojan game in Italy had nothing to do with Homeric Troy, the appearance of two such names, quite unconnected but in circumstances which led the Roman world to derive the name and the practice of the Troia from the city of Troy, is at least a very strange coincidence. So far as certainty is concerned it must remain a coincidence. But there is a suggestion which can be made diffidently. Many places in Europe are called 'Troy', or something very like it, because they were associated with mazes or maze rituals. There were places in Italy called Troy, and it is hard not to think that the reason was the same. That is, in many places and in many languages this name was given from the existence of a maze. It is only necessary to add one place to an already long list, a place which happened to become famous: and to suppose that the land of Troy and later the city itself were called Troy after the Trojan game.

This is a daring proposal,⁴⁷ and it is hardly justifiable if nothing is known about any 'magical walls' or cognate ritual at the city of Troy. But the condition has only to be stated to be admitted. Troy, of all places, had the most famous magic walls in the world. There is certainly no contemporary record that the Trojan game was ever played there, but there are hints that the magic circle was given some treatment of this kind. Achilles dragged Hector at his chariot three times round the walls,⁴⁸ to unwind the magic,⁴⁹ an intention which can be paralleled.⁵⁰ Homer, of course, disagrees, but there are very characteristic reasons for his disagreement. He improved his poem, by his change in the story, successfully enough. Now what Achilles was doing with Hector seems to have been partly understood by a Latin writer, who, having more early Greek literature for reference than we have, said that Achilles 'lustravit' Troy with Hector's body.⁵¹ This means a kind of purification, and perhaps in the opposite sense a kind

⁴⁷ I have made it already in a letter to *The Morning Post* (13 September 1930), and in *Classical Philology*, 1930, xxv, 362, note 3.

⁴⁸ Sophocles, *Ajax*. 1129ff.; Euripides, *Andromache*, 107; Verg. *Aen.* 1, 483, cf. II, 272; Quintus Smyrnaeus, I, 12, 112.

⁴⁹ W. R. Paton in *Classical Review*, 1912, xxvii, 45ff.

⁵⁰ Cf. W. Simpson, *The Buddhist Praying Wheel*, 88ff., 103, 146f., 152, 180, 183, 188, 290f.

⁵¹ *Culex*, 324. Cf. Plin. *N.H.* VIII, 161.

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of exorcism. Achilles intended to work an unfriendly effect on Troy by his circular movements. It is interesting that Seneca⁵² uses the noun from which the verb is formed in writing about the Trojan game, as, according to him, it was played at Troy. He calls it 'sollemne Troici lus sacrum' played 'stato lustris die'—an annual or at least regular performance on a fixed day of 'lustration'. This is of course the friendly sense of the word, and it is applied here, apparently, to a ritual meant to sustain, like an Italian 'amburbium', the effective distinction between the powers of without and within. Seneca affords strong evidence that he used old material now lost, and he probably had sound authority for his strange indication.

At this point it is worth while to remember that the Salian dance has been called a 'Troia on foot', and the Troia itself a kind of 'Pyrrhic' war dance. It is also worth while to remember that Saon, 'the protector', who is said to have invented the Salian dance, and the Sai who first performed it, are recorded to have belonged to Samothrace;⁵³ that in that island the heroic genealogies of Troy and Boeotia, where also there were magic walls, interlock;⁵⁴ and that the Roman king Tarquinius Priscus improved the organization of worship at Rome, being 'mystically imbued with Samothracian cult doctrines'.⁵⁵ The Trojan game was believed to come from Asia Minor, and there is a paradoxical example of this, for though it is generally agreed that the rite had been known in Italy very early, as the Tragliatella vase alone is enough to show, Sulla is often supposed to have introduced it into Italy from its eastern home.⁵⁶

All this is coherent enough, and that is the most that we can expect, when we are in search of a required generalization, and the evidence is scattered and not very articulate. This is not surprising, since the nature of a 'mystery' enjoins reticence on those who believe in it. It is time to return to Bryn Celli Ddu, but on the way back we are met by the question whether any connexion can be suggested between rites and beliefs of classical antiquity and Bronze Age Britain. Such a suggestion exists, though it is naturally hard to demonstrate its

⁵² *Troades* (Teubner text, ed. R. Peiper and G. Richter, Leipzig, 1921), 777ff.

⁵³ Gruppe, vol. I, 199 note 11; and other references at note 39, supra.

⁵⁴ W. R. Halliday in *Liverpool Annals of Archaeology and Anthropology*, 1924, XI, 20.

⁵⁵ D. Servius ad Verg. *Aen.* II, 296; cf. ad 325 and III, 12 (where the Samothracians are said to be akin to the Romans).

⁵⁶ Plutarch, *Cato Minor*, III, is sometimes thought to suggest this: but it is by no means a legitimate inference from what Plutarch says.

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application to megalithic man. It is this. Allcroft⁵⁷ has expressed neat equations between the rites of British tumuli-folk round their tumuli, the worship of the Roman Consus⁵⁸—a *Neptunus equester*—whence developed the horse races of the Roman circus, and the Homeric practice of encircling a new grave with horses. In all these acts peripheral movements, helped apparently by the peculiarly sinister or potent qualities which horses are believed in early thought to possess,⁵⁹ seem to be used to complete a burial. The plan is very like the plan of the Troia. It is again a distinction between the powers of without and within. The practice is related in the same category to the dragging of Hector round Troy, by which Achilles meant to infringe, not to enforce, the same distinction. Even Homer allowed Achilles to drag Hector three times round the barrow of Patroclus. The horses continue the parallel with the Troia. On the Tragliatella vase not only are horses ridden by some participants, but horses are pictured on the shields of those of them who are dismounted. The reason is clear. Horse magic helped to create the needed distinction. Horses taking part in peripheral movements were expected to reinforce the imaginary wall which the movements were thought to create. There is a close parallel. A fairly well defined area exists in which the familiar superstitious beliefs about horses are held,⁶⁰ and south of it these beliefs seem to be applied to other animals, in particular lions.⁶¹ Herodotus⁶² records that the Sardians, who were near the margin between these territories, carried a lion round their city to strengthen the defence.

If Achilles encircled the tomb of Patroclus to complete the act of burial, he was clearly enforcing the distinction between the dead and the living. He, or those who invented the ritual—in so far as such methods can be said to be invented—clearly or dimly understood that this distinction was necessary if the dead were to rest, and, perhaps, if they were to let the living rest, also. At first Patroclus had failed in this respect, for his ghost returned. Here is the arrival back again

⁵⁷ A. Hadrian Allcroft, in *Archaeological Journal*, 1921, LXXVIII, 325ff. Cf. Eitrem, 23, 27.

⁵⁸ Cf. W. F. J. Knight in *Classical Philology*, 1930, XXV, 365, note 4.

⁵⁹ L. Malten in *Arch. Jahrb.*, 1915, XXIX, 179ff. I owe my knowledge of this reference to the kindness of Professor A. D. Nock. I have investigated the incident of the wooden horse, regarded as a magical attack on the wall of Troy, in *Vergil's Troy* (Oxford, 1932), 105ff.

⁶⁰ Malten, 251f., 254.

⁶¹ *Ibid.* 253f.

⁶² I, 84.

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at Bryn Celli Ddu. The burial must be made complete, and an important part was the ritual which should seal, as the walls of Troy were the seal of the city,⁶³ the area of the tomb. The place must be magically shut. But there must be a way in, to bring offerings, and possibly to perform other acts of tendance. So with the exclusion went the correlative principle of conditional penetration, and the maze or spiral is justified.⁶⁴

There is at least something definite which it is fairly safe to say by way of conclusion. The patterns of Bryn Celli Ddu should be considered in a large context of the later history of symbols and their ideas. Induction from examples, subject of course to all the uncertainties of that method, brings us to one generalization: that the maze form habitually implies correlative notions of exclusion and conditional penetration. Deduction then offers us the inference that the same notions, however inchoate and confused, actuated megalithic man. How each of the symbols at Bryn Celli Ddu was meant to work in creating the correlative effects of their magic I do not attempt to decide. It is to be supposed that they were expected to create a field of magical force of the right kind. The pattern stone itself may have been meant as a chart to direct ritual dancing at the foundation; and it may afterwards have been dug into the monument, in the hope that its sympathetic presence might help to maintain the conditions originally intended by

⁶³ The wall was called *ἱερὸν κρήδεμνον*; *κρήδεμνον* means a veil or the seal of a jar, literally a 'head binding'. (W. R. Paton in *Classical Review*, 1913, xxvii, 45).

⁶⁴ For the point of contact between stone circles and foundation rites, cf. together especially M, chap. xvii, *Stone Labyrinths and Rock Engravings*, 147ff., with figs. 124ff, and D. L. Burdick, *Foundation Ceremonies and Some Kindred Rituals* (New York, Abbey Press, 1901), chap. xl, *Circular Movements and Symbols*, 149ff. Burdick's book is unfortunately out of print and I have not found a copy in England. I owe my knowledge of the text of it to the very great kindness of Dr Eugene S. McCartney of the University of Michigan. The comparison of these two chapters strongly suggests that the principle of the magic circle, and ideas of exclusion represented also by maze formations, were of great importance in the plans of the builders of early stone monuments. The Trojan game was itself performed as part of the funeral rites of Anchises, according to Vergil. After its revival, it was sometimes performed at funerals; for example round the graves of Caligula and Drusilla (Dio Cassius LIX, 11). Cf. Pliny, *N.H.* xxxvi, 84: he thinks a labyrinth specially suitable either for a palace or a burial, or—as according to him most people believed—as a structure sacred to the sun. Probably all the suggestions are partly right. Cf. *ibid.* 90ff., where he quotes Varro on the labyrinth at Porsenna's tomb. With 92, where bells are mentioned, many uses of bells apparently defensive may be compared.

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the dance. But it is better not to go too far : for if footsteps should miss a turning in the blind march, there would then be a longer path for them to retrace.

NOTE BY THE EDITOR

Some of the medieval English turf-cut mazes are associated, at any rate topographically, with the walled villages of prehistoric times. The maze on St. Catherine's Hill near Winchester lies within the Iron Age camp on the hill. There was probably a maze between Old Sarum and Salisbury, as the name Mizmaze Hill occurs there on the Ordnance map of 1806-8. Breamore maze in Hampshire is on a hill which is covered with Celtic fields, and is close to a long barrow. It is a long way from the medieval and modern village. If it is true, as is generally believed, that the sites selected by medieval fairs go back to a remote antiquity—Yarnbury and Tan Hill are instances—there may be a significance also in the other associations cited above. It would be interesting to collect and analyze the instances of fields called Troy Town. Do they too occur in 'prehistoric' surroundings?

Notes and News

THE BATTLE OF MOUNT BADON

In this note an effort is made to find a site for the celebrated battle of Mount Badon, in which Arthur is said to have defeated the Saxons, according to one account; the last of twelve great British victories, according to another.

It would be as well first to state the only grounds which can be found for believing in the historicity of Arthur. On the face of it, it is exceedingly doubtful. Arthur is mentioned by name by no author before Nennius in his *Historia Britonum*, written in the middle of the ninth century. (Unless the account of Arthur comes directly from one of the written sources which Nennius copied, and this is a matter of guesswork). He is not mentioned by name by Gildas, who was born in the year of Mount Badon and who does mention the battle. But if Arthur were not an historical character, he must have been an important mythical hero. The one argument which proves that he really existed is that there is no mention of this mythical hero until even later than the History of Nennius. If Arthur had first appeared as a hero and later Mount Badon had been added to his exploits, it would appear to be an obvious example of the common dislike of anonymity in heroes and nothing more than the attempt of someone with no feeling for historical accuracy to find a name for the conqueror of the Saxons. The process is a very usual one, but in fact the process is here reversed. Finally, it has been pointed out that the failure of Gildas to speak of Arthur is not an insuperable difficulty, as Gildas was not given to mentioning historical characters by name until he comes to the princes of his own day, whom he wishes to denounce. (Oman, *England before the Norman Conquest*, p. 211). Gildas' silence, however, does show that it is extremely improbable that Arthur was a ruler over very vast domains.

There are three sources for a consideration of the date of Mount Badon. The first, and clearly the most reliable, is from Gildas, who mentions that the battle was fought on the date of his own birth.*

* Ex eo tempore nunc cives, nunc hostes, vincebant ut in ista gente experiretur Dominus solito more praesentem Israellem, utrum diligit eum, an non; usque ad annum obsessionis Badonici montis, novissimaeque ferme de furciferis non minimae stragis, quique quadragesimus quartus ut novi orditur annus, mense jam uno emenso, qui et meae nativitatis est. Gildas, *De Excidio et Conquestu Britanniae*, 26

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Unfortunately, his style at this point reaches an obscurity deep enough to make him notable as a writer apart from anything he has to say. It is impossible to say for certain whether the 'forty-fourth year with one month added', refers to a period before the writing of the book (*i.e.*, the age of Gildas), or to a period after the appearance of Ambrosius Aurelianus, of which he has been speaking. The first conjecture would almost certainly give us about the year 502. The second itself depends on the date of the appearance of Ambrosius. If we follow Mr Nicholson and take this as 467, Mount Badon would have been fought in 511. If we accept the interpretation suggested in *ANTIQUITY*, June 1931, pp. 238-9, we get 499 for the battle.

The other two sources are far later. The '*Annales Cambriae*',¹ an interpolation in one manuscript of Nennius' History, gives the date as 515, 516 or 518. (The exact date depends on the reckoning of the unspecified year 1 of the Annals, which is either 444, 445 or 447). Nennius gives no date, but it is possible to find some light in a passage immediately preceding the account of Arthur:—'At that time the Saxons greatly increased in Britain, both in strength and numbers. And Octha after the death of his father Hengist, came from the West part of Britain to the Kingdom of Kent and from him have proceeded all the kings of that province. Then Arthur fought against them in those days'. According to the Anglo-Saxon Chronicle Hengist died in 488 and was succeeded by his son, Aesc, who reigned for 24 years, that is till 512. Unless Octha is the same as Ocga, a son of Hengist, mentioned by Florence of Worcester (died in 1118 and certainly used earlier sources in his genealogies) he is probably Octa, who, according to Bede, was the son of Aesc and succeeded him. This again brings us to the second decade of the sixth century.²

It is time to turn to the Saxon accounts, which are two. Bede, who died in 731, copied Gildas. He had, however, his own interpretation of the date of Mount Badon, which he places as 44 years after the coming of the Saxons. This would give us a date about 493. But it is reasonable to suppose that Bede was as much puzzled by the passage as we are, and tried his own hand (a very free one) at its solution. At any rate it is difficult to fit his words:—'*quadragesima circiter et*

¹ Probably late 10th century (Chambers, *Arthur of Britain*, p. 13).

² The answers may be summarized thus: Nennius (9th century) about 512; '*Annales Cambriae*' (10th century) about 516; Gildas (6th century) between 499 and 511.

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quarto anno adventu eorum in Britanniam'—into the text of Gildas, as we should have to do if we supposed that our text is a corrupt one and that he had the original. The Chronicle, of course, does not mention the battle. But this is no argument that it did not exist. Gildas is proof that, whenever it might be, it certainly was fought.

Still the Chronicle is all important. It is the only straightforward account of the invasions and it is obviously necessary to turn to its narrative for this period. The only information of that time is of the movement of the West-Saxons from the South. It is impossible to do justice here to the problems latent in those words and I can do little more than say that I believe that myths of this kind do not grow up out of nothing and that, while I may be ready to jettison Port, the founder of Portsmouth and Wihtgar, the conqueror of Wight, I hold the faith of those who see in Cerdic and Natanleod something more than the inventive genius of a people faced with obscure names to explain. After all, why did they concentrate solely on explaining Netley Marsh and Charford? Why were Downton, Salisbury and Barbury neglected? But it will be obvious in the course of this note that, if the Avon advance is entirely discounted, the whole thesis falls to the ground.

Our first task in examining the Chronicle is to see whether there is any period equivalent to that which is mentioned by Gildas as following Mount Badon, a period during which 'our foreign wars ceased, but not our civil strife'. (Be it noted that Gildas did not claim that the Saxons were driven back into the sea, but only that the wars ceased). It is obvious that there is such a period. From 519, when the Saxons won the battle of Cerdics ford, to 552, when they defeated the Britons at Salisbury, there is a time of consolidation, not of progress. (In 527 comes the mysterious battle of Cerdicsleah, but this did not lead, as far as can be seen, to any further advance in that region). The dates of the Chronicle should not be taken too seriously. In the middle of this period it is noted:—'A.D. 509. This year St. Benedict, the abbot, the father of all monks, went to Heaven'. The date is 34 years out as he died in 543.

But the stay in the advance seems significant. It is surely more than a coincidence that in both British and Saxon accounts there is a halt in the first half of the sixth century.

This study of the dates of the invasion may shed some light on the problem of Bokerly Dyke. It is certain that the Dyke was built after the Roman departure from Britain, (Pitt-Rivers, *Excavations in Cranborne Chase*, III, 14) but so far excavation has given us no *terminus*

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ad quem. The Dyke, however, is a magnificent achievement, as anyone who stands on the summit of Blagdon Hill will admit. It cannot have been built in a day. It needed time for planning. It is possible also that it was part of a system which included Combs Ditch (Sumner, *Local Papers*, pp. 87-107). It may be connected with the Wansdyke, though this is more doubtful. As it stands the Dyke obviously protects from the north and east the populated area of Cranborne Chase and the downs to the south. Mr E. T. Leeds has suggested (HISTORY, July 1925) that it was built to resist a later thrust from the Saxons of the Thames valley through Salisbury. This is, of course, a possible explanation, but it would serve as well to protect this district from an attack coming from further south. From the map it looks as if both assaults would turn its flank, the Salisbury one at its northern end, the Avon one at its southern end. But the Dyke was really a well-planned line guarding a gap between the forests of Cranborne Chase and Holt and to this day its right flank rests on Martin wood, a remnant of the latter forest. An invader from the direction of Charford, if he wished to keep clear of woodland, would have had to cross Bokerley to reach the villages which then stood in the modern Dorsetshire.

But Mount Badon cannot be on the Dyke. It is too far from the scene of the Saxon advance to the Avon valley. As far as can be seen it would be possible for the Britons to have sacrificed the land between the downs above Charford and Blagdon Hill without giving up much of value. (This district was not desolate. There is, for example, the Romano-British farm at Rockbourne. But it was not thickly populated). But once past the Dyke to the west and we enter a country which obviously merited a stern defence. And the Saxon halt after 519 would have given the breathing-space needed for constructing this work.

Of the words in Gildas, 'obsessionis Badonici montis', the second seems much the least important. There is no reason to suppose that the Saxons when they settled on the land would have preserved the British name. This error has led to all the unsuccessful accounts to locate it: Bath, Badbury Rings, Bowne Hill, and so forth. They have nothing else to recommend them and some, such as Bath, are quite impossible. The Mons, however, could not disappear. But if we expect to find a mountain here we shall be disappointed. There is a clue in the Anglo-Saxon translation of Bede ordered by King Alfred (Bede, it will be remembered, copied Gildas), where the words used are *Beadonescan dune*. A down is a likely enough place for a battle in the district near

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Charford on the Avon and it is to the downs above Charford that we should turn.

But, of the three words, 'obsessionis' is the most important. Mount Badon was rather a siege than a battle. There is nothing approaching a Roman or Romanized fort in this district and the Britons must have been holding one of the old hill-top forts of their ancestors. There are two above Charford, both at a short distance. The most imposing is certainly Clearbury Ring, but this is most unlikely as it is too near Salisbury. A far more probable site is Whitsbury camp.³ This has not a very imposing position, but it stands at over 400 feet at the end of a down. Trees cover the fortifications from view until you walk into it, when it will be seen to be a magnificent camp with two ditches, and the walls standing at their highest point 25 feet above the ditch. It is here, I think, that Arthur defeated the Saxons somewhere about the year 510 and gave his people a little rest from the invader.

ROBERT BIRLEY

SOME MONUMENTS OF ARMENIA

Armenia has ever been a land over which battles have been fought, and as the ages have advanced these battles seem to have increased rather than to have diminished, till, with the twentieth century, another and newer type of war has begun—a scientific war of words. But the ardour of the opposing sides is no less great than that shown by Seljuks and Byzantines, by Turks or a Christian minority population. It was over Armenia that the severest struggles of religion were waged; it is over Armenia that the world of art-history has argued of recent years, with unusual vigour, as to the origin of certain systems of architecture and certain types of decoration.

The one side in this argument, led by the eminent Austrian authority Strzygowski, maintains that the dome over a square plan, the whole range of formalistic, non-representational art, the love of such techniques as low relief carving and enamel work, bright colouring and an absence of modelling and half-tones in painting, are to be attributed to the East. The dome above a square plan was, according to

³ Sumner, *Ancient Earthworks in Cranborne Chase*, pp. 20–22, gives an account of Whitsbury camp. He suggests that the raising of the rampart on the southeastern side was carried out by the Britons against the Saxons. But it is difficult to see the use of raising only one part of a rampart or a fort on an open down. The use I have made of this volume, which for anyone who knows the district is an incalculable blessing, will be obvious to those who have read it.

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him, first conceived in the region to the northeast of Persia, where it served as roof to a wooden structure. The transition from the square plan to the circle, necessary for the construction of the dome, was accomplished by laying a beam across the corner and so turning the square into an octagon. By repeating the process, it could be turned into something approaching a circle. Low relief carving, especially a certain type of slant carving (*Kerbschnitt*), was originated in the same region.

These systems travelled south and west, to be adopted in other countries, where the most convenient local material replaced the original wood. Most important of these regions was Armenia, where the dome over square plan and ornament in low relief was developed to a very marked degree. Had it not been for Armenia, Strzygowski maintains, Byzantine architecture, and with it the great domed buildings of the west—St. Peter's and the Gesu at Rome, even St. Paul's in London—would never have been built, while St. Sophia at Constantinople, the mightiest domed building of the world, would have been a wooden roofed basilica or at most a domed building of circular plan, like the Pantheon.

In opposition to these Eastern theories are those of Rivoira, who attributes everything to Rome; and those of the more conservative school, who admit eastern influence, but attribute the initiative and development to Hellenistic culture.¹ And be it said at the outset that there is little more than brilliant theory and reasoning to justify the oriental theory to the full, for in Armenia and the East there are no surviving buildings as old as those in the Mediterranean region. As regards low relief carving and the accompanying non-representational systems of ornament, however, all authorities now concur in admitting eastern influence, and even if Armenia did not originate them, the systems were certainly developed to a very great extent in that country. A glance at the pillar preserved in the court of the old patriarchate at Etchmiadzin proves the popularity of such ornament at quite a late date (pl. 1). The cross has become the main motive, though the interlaced patterns which surround and adorn it are to be traced back to a date much earlier than the birth of Christianity, when they formed the basis of an art which was universal in the Near and Middle East. In

¹ For a summary of the discussion see Dalton, *East Christian Art*, ch. 1 and 2. For the eastern point of view, see Strzygowski, *Eastern Origin of Christian Church Art*; for the western: Rivoira, *Moslem Architecture*.

PLATE I



PILLAR (16TH CENT.) OUTSIDE THE LIBRARY, ETCHMIADZIN
Pls. I-VII, *ph.* O. G. S. Crawford

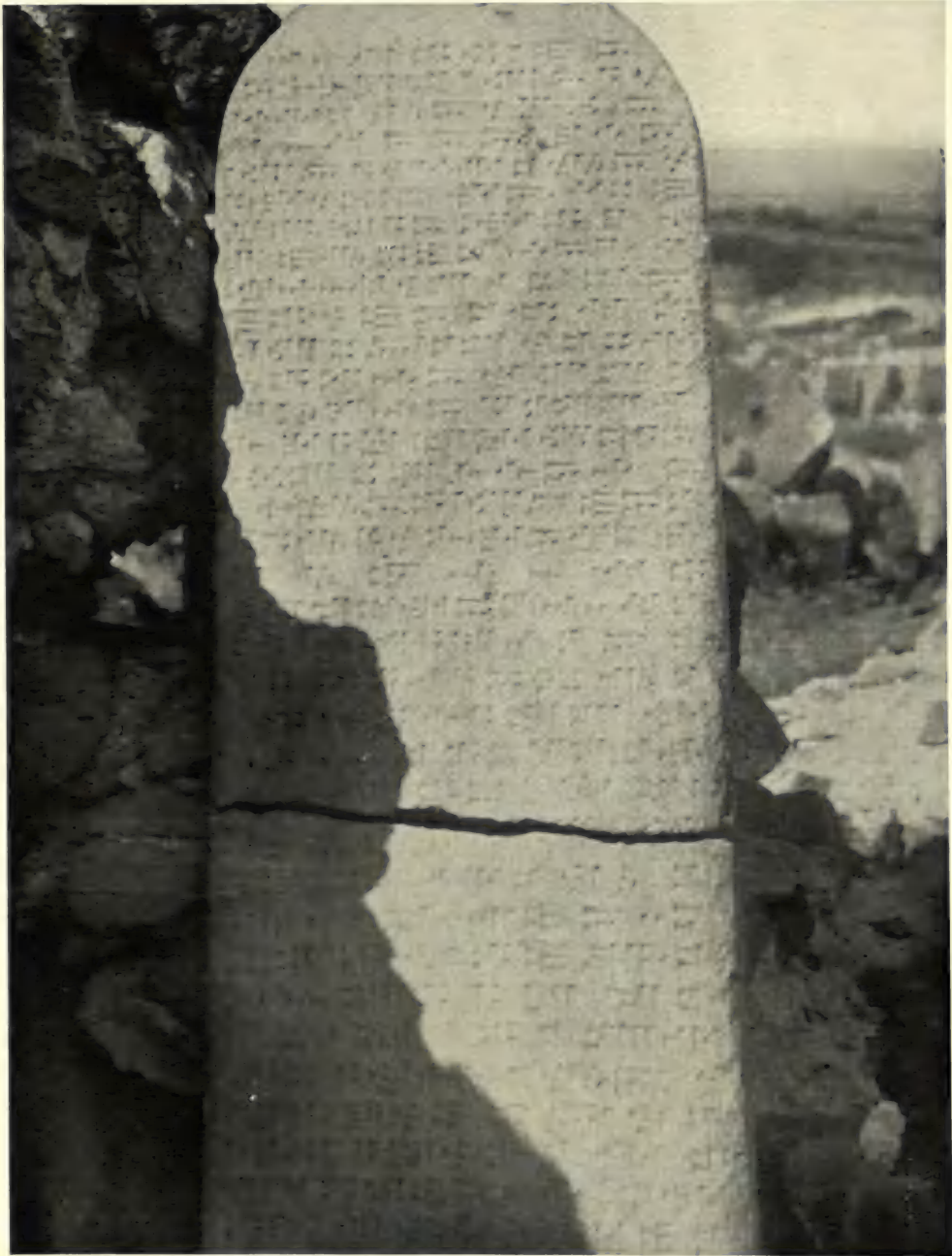
facing p. 464

PLATE II



CAPITALS (7TH CENT.) AT ZWARTKOPS

PLATE III



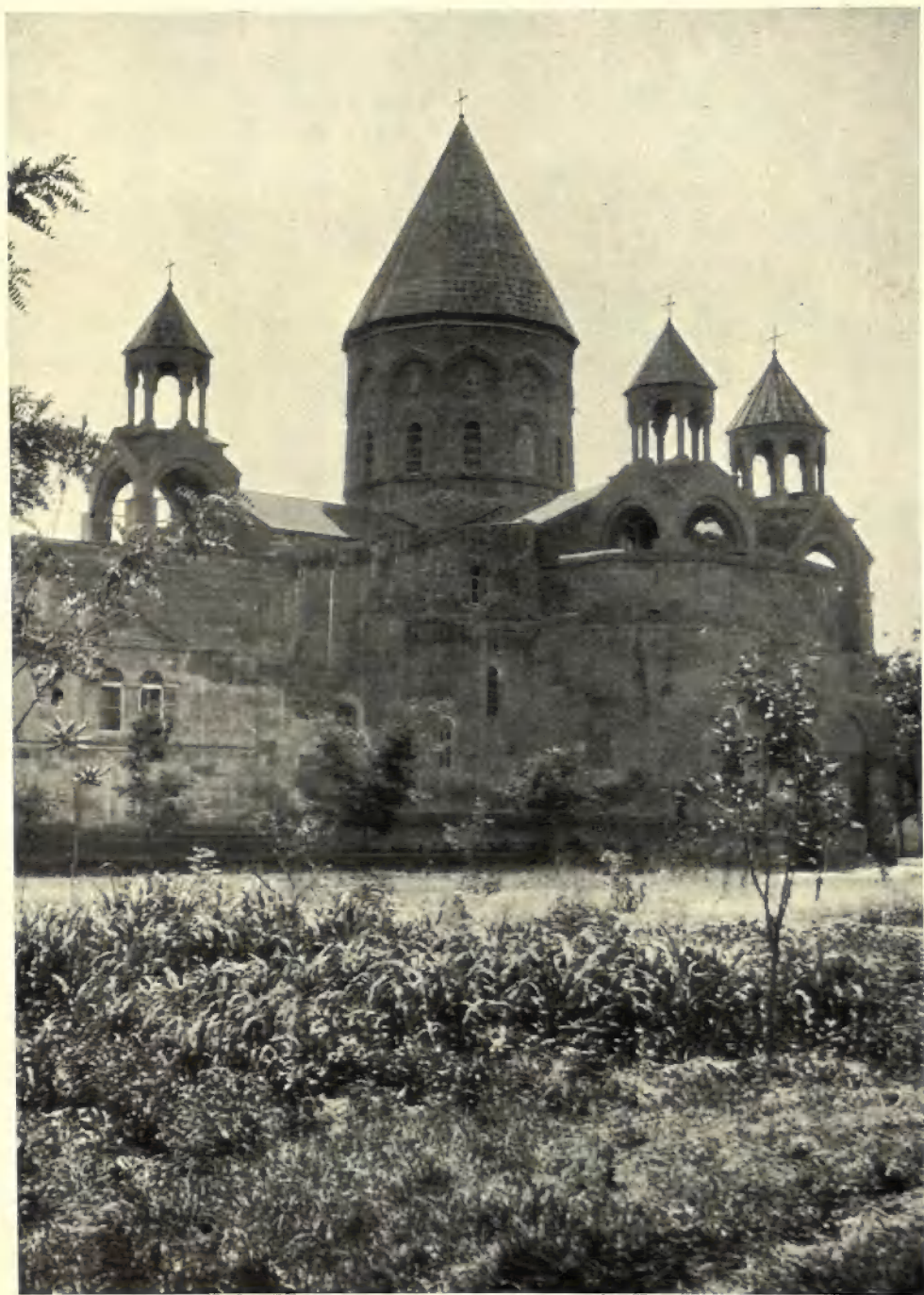
CUNEIFORM STELE, ZWARTHNOTZ

PLATE IV



QUADRANGLE OF MONASTERY AT ETCHMIADZIN

PLATE V



THE CATHEDRAL, ETCHMIADZIN : THE BUILDING 7TH CENT., THE DOME 13TH CENT.

PLATE VI



WEST PORCH OF THE CATHEDRAL, ETCHMIADZIN (17TH CENT.)

PLATE VII



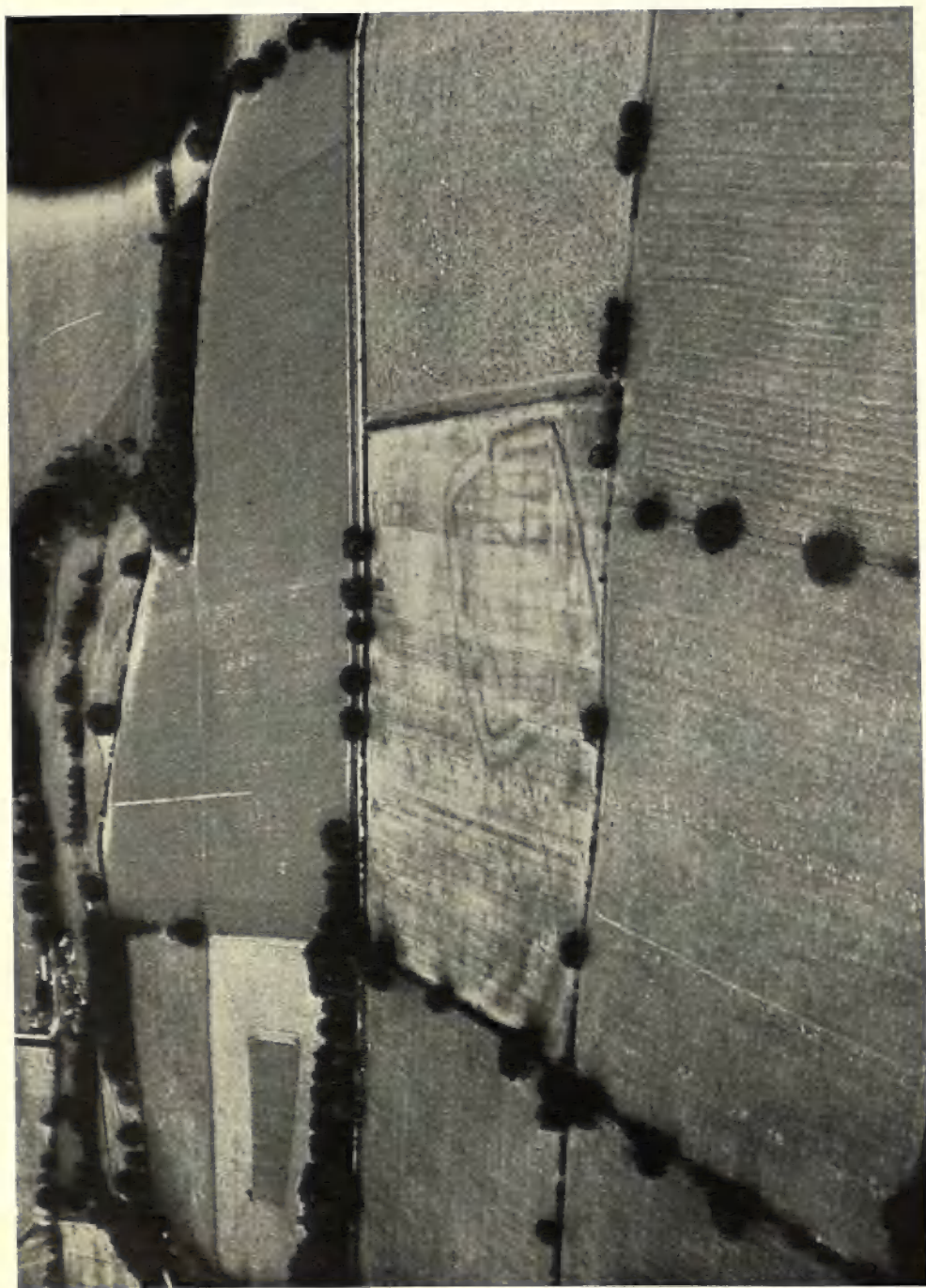
FORTIFIED CHURCH OF ST. RHIPSIMA, ETCHMIADZIN (AFTER 7TH CENT.)

PLATE VIII



WALL OF ROMAN FORT, THIEFSIDE HILL, CUMBERLAND
(See p. 466)

PLATE IX



AIR-PHOTOGRAPH OF ROMAN SITE, THIEFSIDE HILL, CUMBERLAND
Crown copyright reserved

PLATE X



CHURCH AT NICOSIA, CYPRUS

Northwest corner of west front, showing doorway to north chapel and moulded jambs of west door on right
(See p. 469)

PLATE XI



YARNBURY CAMP

Section of bank showing four holes presumably for supporting upright timbers for the revetment of the chalk wall. The holes were 2-3 ft. deep from the original surface, 12-15 ins. diameter, and about 3 ft. apart. The old surface-line is shown and it can be seen where it was broken through in digging the next hole (partially under the bank and not re-excavated). (See p. 471)

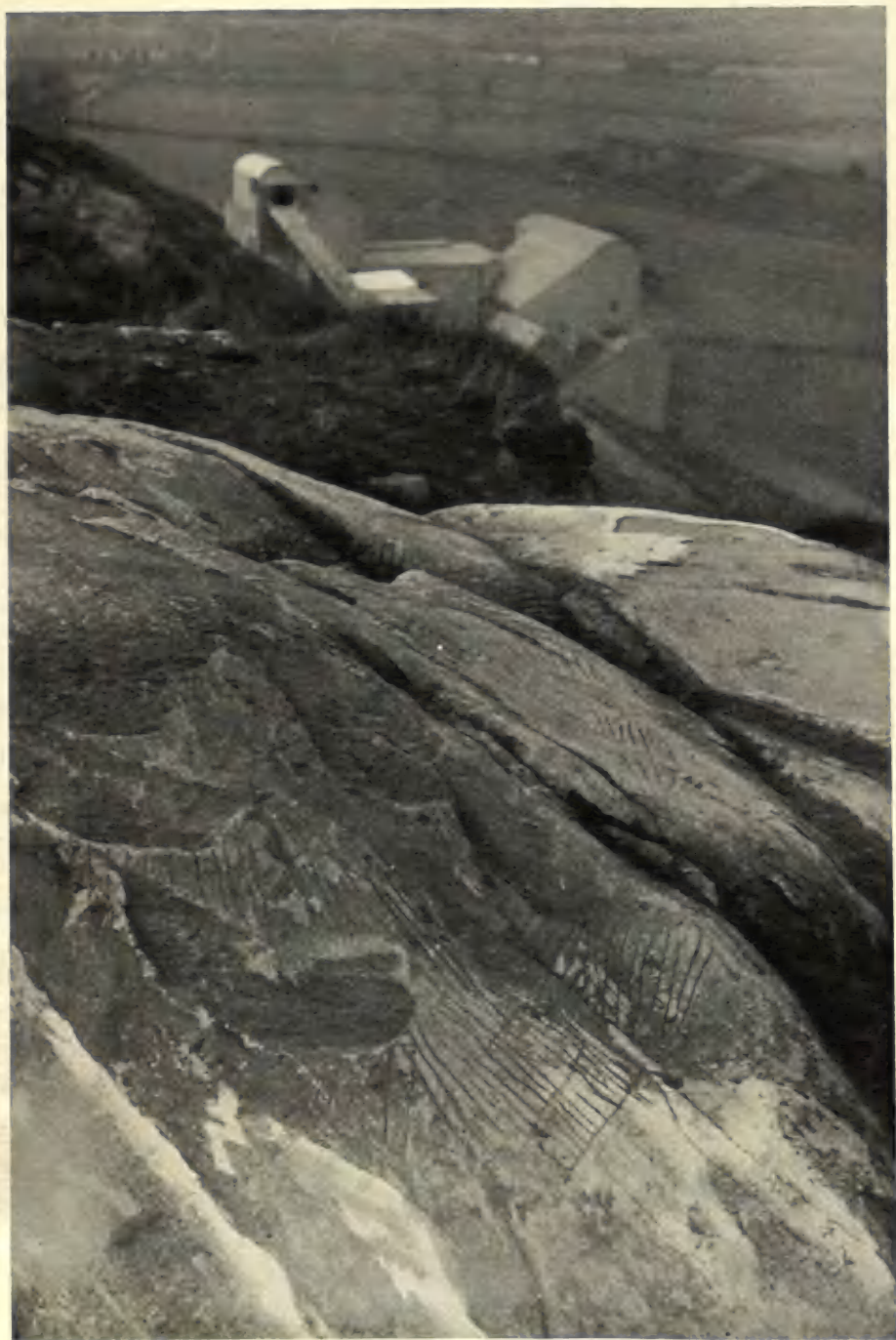
Ph. W. E. V. Young

PLATE XII



HORN FOUND AT ROMSEY, HANTS, NOVEMBER 1931
(See p. 474)

PLATE XIII



SCULPTURED ROCK, TRAPRAIN LAW, HADDINGTONSHIRE, SCOTLAND
(See p. 474)

PLATE XIV



SCULPTURED ROCK, TRAPRAIN LAW, HADDINGTONSHIRE, SCOTLAND



HEMBURY FORT: STAKES RESTORED IN THEIR ORIGINAL HOLES
(See p. 475)

PLATE XVI



BALLOON PHOTOGRAPH OF MEGIDDO

Showing the Town Gate and the area to the south and west. (*See* p. 512)

Copyright Oriental Institute of the University of Chicago

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early Christian times this art was developed to the full, and we find crosses which are closely allied to the Armenian ones in the sixth and seventh century Nestorian churches of lower Mesopotamia.²

None of the actual churches that survive in Armenia are much older than the seventh century, though they may reproduce earlier plans. One of the most important of them, the ruined church of Zwarthnotz (641-661), between Etchmiadzin and Erivan, has a quatrefoil plan, surrounded by a circular ambulatory. For Strzygowski it is a monument due to Armenia and the East. Rivoira would assign it to Rome, while M. Blochet, in his recent book on Moslem painting, seeks to attribute it, together with everything in East or West, of whatever style, to ancient Greece. Hellenistic, or rather Byzantine, influence at Zwarthnotz is not to be denied, however, for some of the capitals bear Greek monograms (pl. II). The low carving is Eastern; the basket form of capital (that to the left) was developed in Mesopotamia before it came to the West, while the spread-eagle (on that to the right) is a motive which is probably to be traced back to Hittite Asia Minor, whence Byzantine and Western examples are equally derived. But whether these influences came to Zwarthnotz direct from the East, or from the East by way of Byzantium, is a question over which the pundits must squabble until extensive excavations dealing with the early Christian period have been undertaken in Armenia. A large stele with cuneiform inscription, now preserved within the Zwarthnotz enclosure (pl. III), points to Eastern influence at an earlier date, and shows that the part of Armenia which lies to the north of the Araxes was formerly under the sway of the powerful state of Van, between that river and the Tigris.

Some of the most famous Armenian churches, most notably those of Ani and that on the Island of Achthamar, Lake Van, are now in Turkish, not in Russian Armenia, and as much of the region is depopulated, access to them is by no means easy. Hundreds of others lie scattered over this once prosperous country, more of them on the Russian than on the Turkish side of the frontier and Etchmiadzin—Vagharshapat is the traditional name—which was before the war the home of the Patriarch and the 'Rome' of the Armenian Church, was naturally well supplied. With the exception of the cathedral, her

² See the author's article on Excavations at Hira in *ANTIQUITY*, September 1932. The interlaced ornament of Armenia is discussed in detail by Baltrusaitis, *L'Art médiéval en Géorgie et en Arménie*. (Paris, 1930).

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churches are now deserted and even the cathedral is destitute of its patriarch and is served by only a few monks, who inhabit a corner of the buildings of the old monastery and theological college (pl. iv). An important museum of christian archaeology which was attached to this institution has been moved to Erivan ; the library survives, though it has apparently been deprived of its greatest treasures, the famous Etchmiadzin Gospels, dated 986 and the ivory cover of the sixth century.³

The monastery of Etchmiadzin was founded in the fourth century. The present buildings are fairly recent, but it appears that they follow a seventh century plan fairly closely, enclosing a quadrangle like an Oxford or Cambridge college, with the cathedral in the middle (pl. v). It has been suggested that this building is to be assigned in part to the fourth century, but it would seem that the walls cannot really be older than the seventh, when the whole edifice was restored. The dome is probably of the thirteenth century, while the elaborate porch is a seventeenth century addition which follows closely in the traditional style of Armenia (pl. vi).

The other churches of Etchmiadzin, if less famous, were little less imposing. They still stand, but their future lies in the balance. One of them, St. Rhipsimas (pl. vii), which is later than the seventh century, if not actually used, is at least kept in repair. Rivoira sees here Byzantine influence; for Strzygowski it is an Eastern building of the type from which Byzantium learnt so much. D. TALBOT RICE.

A NEW ROMAN SITE IN CUMBERLAND

In *ANTIQUITY* for December 1930, pp. 472-77, Mr R. G. Collingwood described a newly discovered Roman signal-station on Barrock Fell, Cumberland, which had been explored by Mr R. E. Porter, Dr W. Goodchild and himself. This station had not been suspected until it was recognized and photographed from the air in July 1930, by Wing-Commander Insall, v.c. On the same day he also photographed several other sites in the neighbourhood and the photographs were sent by the Editor of *ANTIQUITY* to Mr Porter, who, with the help of Dr Goodchild, was able to identify the sites on the ground. One of the photographs, which showed a quadrilateral enclosure with double outline in a field of oats, was located on the northwest slope of Thieftside

³ I tried to ascertain their whereabouts when at Etchmiadzin in September 1930, but they seemed to have been mysteriously mislaid between Moscow and Armenia.

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Hill on Petteril Green Farm, roughly midway between Carlisle and Penrith.

On visiting the site Mr Porter and Dr Goodchild found the growth of corn more lush in the area indicated by the dark lines on the photograph and in parts it was laid, but nothing definite could be made out by inspection. Permission was obtained to explore the site and in August 1931 two trenches were cut by Mr Porter and Dr Goodchild. One, about 50 feet long and varying in depth to 5 feet was cut in the northeast corner of the field, but no trace of the enclosure was discovered. About 20 feet east of the outer line of the enclosure a small piece of dark coarse pottery, of a type difficult to date, was found 18 inches below the surface. In the second trench, which was cut opposite the opening in the north boundary of the enclosure, a considerable amount of charcoal was found a foot below the surface. Although nothing conclusive was found on this occasion, owing to the short time available for exploration, it was felt that the site was sufficiently important to demand fuller investigation, and arrangements were made to renew the exploration in July 1932, after the hay crop had been gathered.

The field slopes gently towards the west with slight undulations, but no indication of the underlying camp could be observed on the surface. We were informed by the tenant, Mr Savage, who has farmed the land for eight years, that occasionally during ploughing a large stone has been encountered but he had not seen any evidence of walls.

It was decided that the most promising portion of the site was the northeast angle, which showed up so clearly in the photograph. A trench was cut at right angles to the north fence, commencing about 20 feet away from it. Twenty-nine feet from the fence stones were encountered 18 inches below the surface, which, on being cleared, were found to belong to a wall 18 inches high, built of three courses of boulders, the lower course of which had been laid in a shallow trench and embedded in clay. (Plate II). This wall, which was found to run towards the east, was followed to the end of the curve and the direction of the north and east boundaries of the enclosure was proved by trenches cut at a short distance away from the angle. The trench was then continued in a southwesterly direction, and at a distance of 28 feet from the outer wall a second wall consisting of a single course of smaller stones was found running parallel to the outer wall. This inner wall was also proved on the north and east sides of the enclosures, where it was also 28 feet away from the outer wall.

In the air-photograph (plate I) the outer boundary of the enclosure

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is seen to pass under the east fence of the field. A trench was cut at this point and the outer wall was found at the point where it passes under the fence, while the inner wall was 26 feet away from it. By siting from trees which could be recognized in the field fences, the approximate situation of the southwest and northwest angles were located on the ground and their position determined by trenching, but no attempt to prove the inner wall was made at these points. These excavations were sufficient to determine the area of the camp and attention was then turned to the gap in the north boundary. The wall on the east side of the opening was located, but lying close behind it was a modern stone-built field drain. The wall at this point consisted of a single layer of stones, two feet wide, bedded in clay, and it was thought that it had been robbed during the construction of the field drain. The wall terminated by turning south towards the enclosure. About 5 feet from the end of the wall was a paved causeway, 19 feet wide, formed of well laid stones bedded in clay. To the west of the causeway the termination of the wall was found to consist of a broad foundation of stone, but lying to the south of the line of the eastern portion. From this arrangement it was thought that the entrance to the camp had been guarded by an internal clavicula running from the western side of the opening, but no trace of such could be found.

The site is apparently a semi-permanent camp which was protected by a clay rampart 28 feet wide at the base with retaining toe walls externally and internally. The outer toe wall was 18 to 24 inches broad and at the northeast corner 18 inches high, while the inner wall was much lighter and less well constructed. At the northeast and southeast angles, and at the abutment of the wall on the gateway, the foundation appears to have been strengthened by a roughly pitched layer of stone between the toe walls on which the clay rampart had been built. Whether this foundation also existed at other points between the toe walls was not proved. The soil of the field is a stiff red clay in which are erratic boulders of Criffel granite, sandstone, and metamorphic slate, of which the toe walls were constructed. In shape the camp is an irregular quadrilateral, the outside measurement of the sides varying from 300 to 354 feet. The east, south, and west sides are straight, but the north side shows a marked curve and the portions on either side of the gate are not in alignment. The area of the enclosure within the rampart is approximately $1\frac{3}{4}$ acres.

The only finds made during the excavations were a small piece of dark coarse pottery and a fragment of tile about two inches square

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and one and a half inches thick, which are insufficient to assist in assigning a date to the camp, but from its construction it probably belongs to the latter part of the first century.

Although the trench outside the northeast angle was continued up to the fence no trace of a ditch was discovered. This trench was only cut down to the level of the footing on the outer wall but throughout its length the soil appeared to be hard and undisturbed. Clay of this type, after a long interval, would become very compact, and indications of a ditch might be easily overlooked.

On the air-photograph there are indications of a road 350 yards east of the camp running in a northwesterly direction from the summit of Thiefside Hill towards the bridge over Blackrack Beck. On the south side of the hill a sled-track from the quarry on the summit continues the line of this road, its direction being straight towards the Roman station of Voreda, two and a half miles to the south. Towards the northwest the road, if continued in a straight line, would strike Barrock Fell—three miles away—approximately where the signal-station was located two years ago. This road, which, owing to the limited time at our disposal, was not trenched, is probably an early road, the present main road from Penrith to Carlisle, being constructed later on the east side of Thiefside Hill to avoid the steep ascent. It is highly probable that there was a signal-station on the summit of Thiefside Hill connecting the Barrack Fell station with Voreda but, if such a station did exist, all trace of it has been destroyed by extensive quarrying on the summit of the hill.

We are greatly indebted to Mr Foster Savage of Petteril Green farm for permission to explore the site and for an invitation to continue the exploration in the autumn of 1934 when the field will be in stubble after corn, and also to the trustees of the late Miss Parker of Penrith. The expenses in connexion with the excavations were defrayed by a grant from the Research Fund of the Cumberland and Westmorland Archaeological and Antiquarian Society.

J. E. SPENCE.

A THIRTEENTH CENTURY CHURCH AT NICOSIA, CYPRUS

During recent alterations, which entailed the lowering of the sixteenth century ramparts north of the Famagusta Gate, the walls and foundations of a thirteenth century Latin church have been discovered. They have, since, been cleared by the Municipality under the supervision of the Cyprus Museum, and the site fenced in.

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The small church is of the early Basilican type with apse at the east end. The apse was cut away to make room for the stone face and the whole church razed to the ground and levelled off, to make a platform for the rampart which was raised some 20 feet above the church. The roof was vaulted, for all the ribstones were found to have fallen in their relative positions and the two bosses, one with the cross of Jerusalem and the other the 'Agnus Dei' of the Templars, were intact.

The north wall still stands to 5 feet 6 inches and has been plastered with white gypsum at a period subsequent to its erection. A painted panel is of later date and shows the lower part of a draped figure on a black background; the border is in red. To the left is one of the pillars from which the ribs of the vault sprang. The south wall fell outwards, and only two courses remain standing; among the débris most of the tracery of a 'Flamboyant' Gothic window was recovered together with a great deal of carved stone.

The west end of the church (plate x) is badly destroyed, but part of the carved jambs of the door may still be seen and the flight of steep steps which lead up to it. An octagonal tower supports the southwest corner and may have been a belfry. The steps end abruptly before reaching the tower and a small chamber of later construction juts out from them, round the foot of the tower. On its west wall two steps remain which seem to suggest that it is the basis of a stairway. On the north side another chapel adjoins the church: it extends beyond the west front and is larger than the church. The main doorway leads on to a flight of semicircular steps which join on to those of the west door; this chapel is rectangular and is also connected to the main church by a small door. At the east end traces remain of an altar tomb: the surrounding wall with opening at the back stands a few inches in height, and the bottom is lined with cement. Two steps lead up to it.

Half way down the church projecting from the north and south walls are two tomb-like structures—the one to the south is still covered with a large slab of local marble, but has no inscription. Of the other, only part of the solid masonry base remains. Between them, two shallow steps lead up to the altar.

The floor of the building, which was some 3 feet 6 inches above ground level, is of marmora in fairly good condition. It was covered with masses of building rubbish, tiles, stones—a number still having the painted plaster face with figures of saints in brilliant colours, and much gold; also much plaster tracery with the shaped glass roundels of windows fitted to them. Above this, a large quantity of earth had

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been shot to level off the site. It contained much medieval glazed ware and a few coins belonging to the fourteenth century.

The little church appears to have stood on a natural eminence and must have been one of the many which were destroyed at the time the Venetians began the ramparts (designed by Savorgnano) in 1567, and probably belongs to the period when many Europeans flocked to the island and much of the best building was done. It is hoped that part of the building may be roofed in, and the material found on the site exhibited there.

JOAN DU PLAT TAYLOR, Asst. Curator, Cyprus Museum.

YARNBURY CASTLE

Mrs M. E. CUNNINGTON has sent the following note on the date of the inner work at Yarnbury Castle, Wilts. :—

Yarnbury Castle on Salisbury Plain is a fine plateau camp about 28½ acres in extent, with triple entrenchments and an elaborately defended entrance on the eastern side. There are also gaps through the ramparts on the north and west, either of which may possibly be original, in spite of the fact that the ditches seem continuous at these points. It is unlikely that such a large enclosure would have had only one entrance.

One of the most interesting features connected with the site is an inner entrenchment, roughly concentric with the outer, consisting of a much defaced single bank and ditch. It was thought that this inner work represents an old earthwork of the 'causewayed' or 'interrupted' ditch type, like that on Windmill Hill and Knap Hill, in Wiltshire, and the Trundle and Whitehawk camp in Sussex. On the ground there were high places in the filled-up ditch that somewhat resembled causeways, and these appearances were well shown up in photographs taken from the air.

Excavations, extending over three weeks, were undertaken in June last, solely with the purpose of testing the character and date of the inner work, and with the expectation, as well as with the hope, of proving it to be of the causewayed type.

Though it is plainly seen on the air-photograph¹ it is difficult to

¹ One of these, with a general description of the site, was published in Crawford and Keiller's *Wessex from the Air*, 1928, pp. 68-71. It was reproduced in *ANTIQUITY*, March 1930, with an article on Neolithic camps. In both these publications attention was drawn to the inner ring with its apparent causeways, which are said to have stood the test of the boser.

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trace the inner work on the ground, except on the west and southwest sides, and here the only three visible 'causeways' were tested. In two of these the ditch was continuous, and the appearance of a causeway being due to unequal filling up of the ditch; the third proved to be indeed solid, and a normal entrance way, 15 feet wide, with flanking ends to the ditch, and post-holes within—in all probability those of the gates.

Suspensions as to the date of the earthwork were aroused almost as soon as digging began, on account of the scarcity of flint flakes and of worked flints of any kind. Apart from the pottery found in it, as soon as the first section of the ditch was cleared out there could no longer be any question of its being Neolithic. The ditch was well and regularly cut and, as originally made, from 11 feet to 12 feet deep, and nowhere more than a foot wide at the bottom; so this inner work, though only a single bank and ditch, must have been of considerable strength.

Large numbers of sherds of Romano-British pottery were found in the superficial layers, and Iron Age pottery beneath to the actual bottom of the ditch.

The earliest type of pottery is that of a red haematite coated cordoned bowl, similar in every respect to some from the Hallstatt-La Tène village site at All Cannings Cross. Haematite coated bowls with horizontal furrowing such as were most common at All Cannings are not represented, and none of the coarser wares show finger-tip ornament.²

Several other Wiltshire sites have yielded sherds of haematite coated cordoned bowls, and not of the furrowed, and there is reason to believe that as a type the cordoned bowl is later than the furrowed. The sherds of cordoned bowls are ornamented with chevron pattern and other lines, and as on all other Wiltshire sites known to the writer the ornament is scratched through the colouring AFTER firing.

The absence of finger-tip pottery, and the presence of haematite coated bowls, seems to justify the conclusion that the inner earthwork is rather later in date than the All Cannings Cross village, or at earliest contemporary with the end of the occupation there, and may be assigned therefore to the early La Tène period.

The date of the great outer entrenchments at Yarnbury is not

² M. E. Cunnington, *All Cannings Cross Farm*, plate 28, figs. 1-5. (G. Simpson, *Devizes*, 1923).

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known ; it is probable, however, that they are late La Tène, but the possibility of their being of the post-Roman dark ages should not altogether be ignored, though the fact that nothing recognizably later than the Roman period was found does not lend probability to the later date.

Sections were made in what remains of the bank, both to the north and south of the causeway, and in each was found a row of post-holes which it seems could only have served to hold the posts of a timber

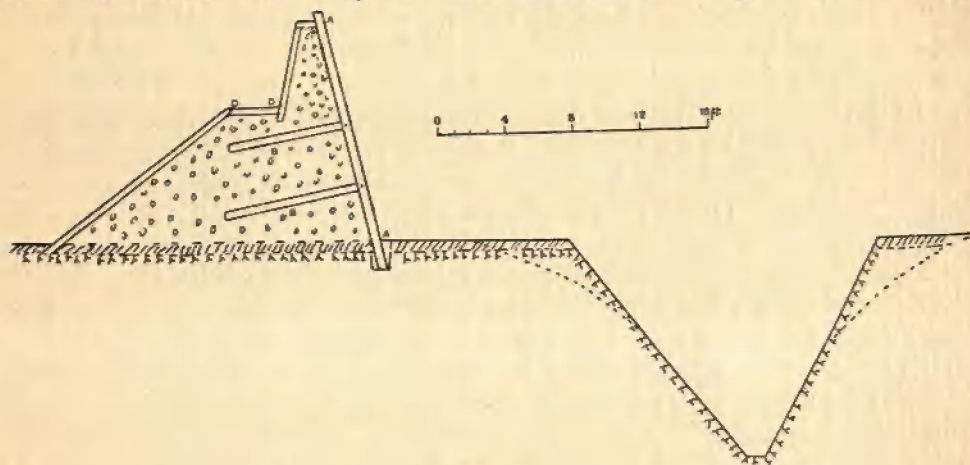


FIG. 1

Section of ditch, with dotted line indicating the profile as altered by weathering, and a conjectural section of the wall as supported by timber revetment posts (A-A) standing in the holes found under the bank ; these posts could have been tied back by horizontal poles embedded in the chalk core (B-B). Probably the whole outer face of the wall was masked by upright timbers, but only every two or three were strengthened by the butts being sunk into holes below the turf level. Poles laid horizontally behind the uprights would have afforded a possible foothold for climbing, and for this reason were no doubt regarded as undesirable. It was necessary to place the wall some way back from the ditch to allow for the inevitable weathering of the edge of the ditch. It would have been quite feasible to construct a rampart walk on the inner side of this chalk and timber wall (D-D).

We are indebted to Lieut.-Colonel R. H. Cunningham, R.E. (retired) for details of the conjectural reconstruction of the chalk and timber wall.

revetment to support and mask the chalk core of the walls. (See plate XI and fig. 1).

As a result of the excavations it seems that sometime early in the Iron Age a single entrenched earthwork of considerable strength was made, with an entrance to the west, and probably another to the east. Sometime later, but probably also in the pre-Roman Iron Age, the present existing triple entrenchments were made, perhaps because a larger enclosed area was needed or because the older work was of an obsolete type, and possibly out of repair. The filling-in of the ditch was of an unusual nature, but it seems for the most part to have been due to natural causes, and not to intentional filling up, so it is quite

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likely that the defences were in bad repair before the outer works were made. The inner bank, however, was certainly partly demolished intentionally, by being thrown inwards, thus levelling the inner area to some extent, and making the remnant of the bank appear less than it actually is. Whether the site was occupied continuously from the time when the first entrenchment was made, right through into the Roman period, it is hardly possible at present to determine, but that it continued to be inhabited after the inner ditch was filled up (almost to its present level) there is no doubt, because several pits, as well as a grave, were found dug partly in the silt and partly in the solid bank. One large pit was particularly interesting as it was dug wholly in the ditch near the centre, its outline being clearly marked off from the silt, showing that the pit was dug when the ditch was, within a very few inches, as full of silt as it is today. The pit was of either late pre-Roman or early Romano-British date. A full account of the excavations will be published in the *Wiltshire Archaeological Magazine*.

THE ROMSEY HORN

The horn illustrated on plate XII was found in November 1931 during the construction of filter-beds for the Romsey drainage system, at an alleged depth of between 16 and 17 feet, at the foot of Green Hill. The excavation was filled in immediately afterwards, before any examination of the soil could be made. Another is said to have been found at the same time, but all efforts to trace it have been unsuccessful. It is said to have been similarly marked. The discovery is due to the Rev. S. T. Percival, Rector of Farleigh Chamberlayne, who rescued from oblivion and loss what is evidently a most important object. The present note is merely a preliminary one; the object itself has not yet been examined by experts; but there seems to be general agreement amongst those who have seen the photograph that it belongs to a pre-neolithic date, and perhaps even to the late palaeolithic period. The Abbé Breuil informed the writer of this note that similar types are found in the Magdalenian period.

SCULPTURED ROCK AT TRAPRAIN LAW

The name of Traprain Law is familiar to all archaeologists, for it is here that the famous hoard of silver objects was found 13 years ago.* The 'acropolis', as we might call it, was inhabited at least as

* May 1919. See *Procs. Soc. of Antiq. of Scotland*, 1919-20, LIV, 102 ff, and succeeding volumes; A. O. Curle, *The Treasure of Traprain*, 1923.

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early as the Bronze Age and as late as the fifth century A.D. The most recent discovery suggests a yet earlier period, for the nearest analogies are found in the prehistoric flint-mines of Sussex.† It is, however, sad to learn that it occurred during operations being made 'preparatory to quarrying'. Must all our finest historical and scenic landmarks be sacrificed to the greed of private enterprise? The photographs (plates XIII, XIV) reproduced are different views of a sculptured rock on the top of Traprain Law.

Mr A. J. H. Edwards, of the National Museum of Scotland, observes:—'Nearly the whole of the surface, about 36 square feet, is covered with markings incised and pecked in the softer skin of the whinstone rock. In addition to the well-known cup and ring markings of the Bronze Age there are a number of others of most unusual character. These are in the form of grids and parallel lines intersected by other lines. A number are distinctly tectiform and others again may be highly conventionalized human figures.

'At one side of the rock there is a well-defined carving of a fourteenth century calvary cross (not shown in the illustration). The traditional association of the rock with pagan ritual, or perhaps the accidental uncovering of one of the higher portions with its strange markings, has led to someone saining it with the christian emblem so as to avert evil.

'A cast of the entire surface of the rock was made under the supervision of H.M. Office of Works and presented to the National Museum of Antiquities of Scotland, where it is now on view. A complete description will be published in next year's issue of the Proceedings of the Society of Antiquaries of Scotland'.

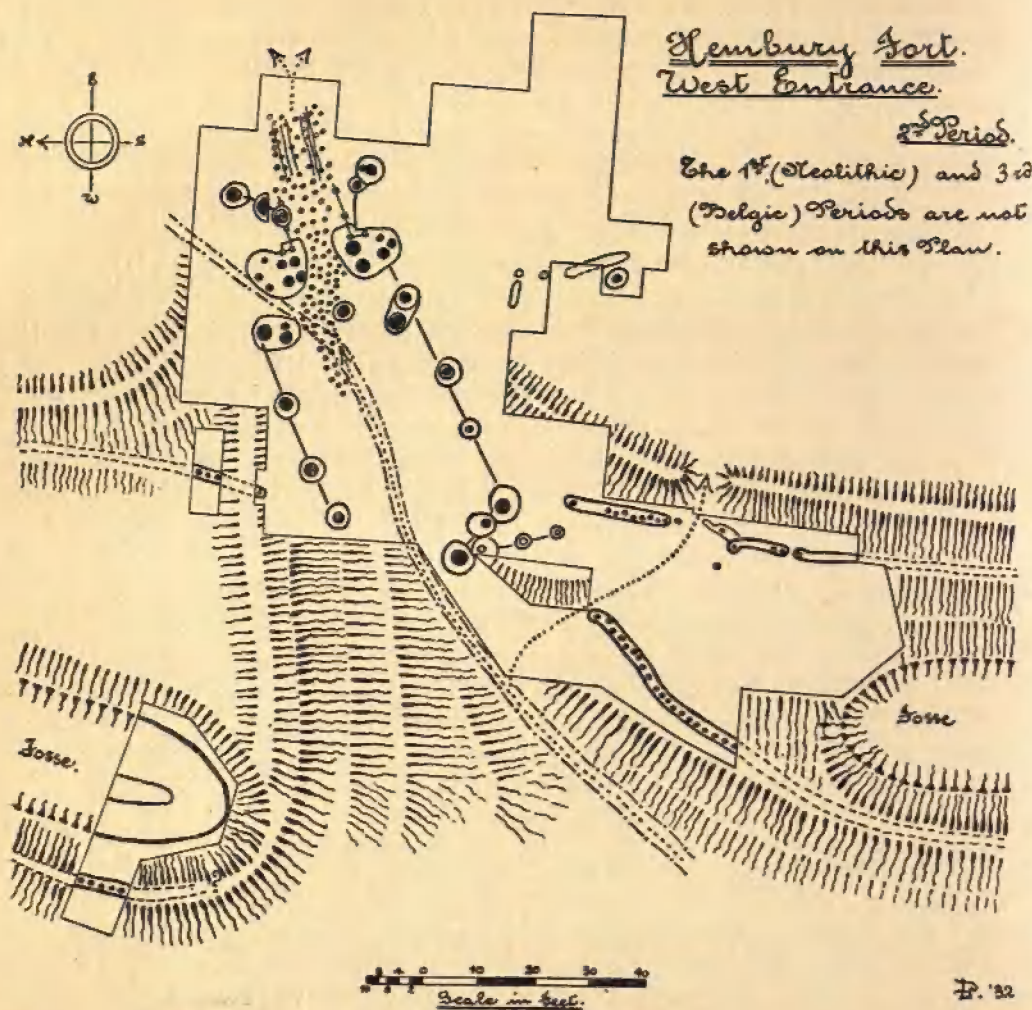
THE PALISADE AT HEMBURY FORT.

Miss DOROTHY M. LIDDELL writes:—The palisade uncovered during the Devon Archaeological Exploration Society's recent excavations at Hembury Fort has a two-fold interest on account of the technique of its construction, which is totally different from that of the examples excavated in chalk sites, and its position in the general scheme of defence with regard to the main gateway.

Four sections were cleared in which the original packing-stones were found still so securely wedged in place in the bedding-trench, that it was possible not only to see the exact size and position of the posts,

† See *Sussex Archaeological Collections*, 1924, LXV, 69-111 and 1926, LXVII, 103-38.

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- Excavated areas within continuous lines
- • Postholes • Ditches • Palisade Trench
- • Cores of decomposed wood posts. • Cobbled roadway with wheel-tracks.
- — — — — Modern Roadway, - - - - - Ancient Roadways.
- Presumed Route of Palisade.

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but to reconstruct a portion of the stockade by slipping new stakes into the original holes, thus giving some impression of its appearance when standing. (Plate xv). The sockets held posts 7 to 8 inches in diameter placed 7 to 9 inches apart, sunk in a trench 2 feet below the surface and packed into place with flat slabs of local chert. In two places these holes were slightly staggered, suggesting that the uprights had been laced with withies or brushwood.

The first stockade is placed well outside the inner vallum but the second is in the centre of the outer bank, beneath its apex. There is evidence that the inner palisade completely encircled the camp—that the outer one did likewise is probable but has yet to be proved.

At the main entrance to the Fort the recurved vallum is flanked by a double row of huge posts (evidently part of a revetment) leading up to a complicated gateway, through which, on the original cobbled surface of the road, wheel tracks are clearly visible.

The palisade comes up to the back of the recurved bank, stopping 10 feet short of the post holes on either side.

Just south of the main gate a hollowed pathway branches from the road and runs up to a gap 11 feet wide in the palisade—a postern, or sally-port. This is sheltered by the palisade from the second vallum which, curving round the end of the great ditch, forms a protecting screen to this secondary entrance. Twenty definite post-sockets were exposed in this section.

That a large part of this Iron Age work is built upon the silted up Neolithic ditch belongs to another chapter in the story of Hembury Fort.

GAZA

Sir FLINDERS PETRIE writes:—‘The discovery of the five palaces of Gaza has given a firm connexion with Egyptian history from the XVIIIth to the VIIIth dynasty (1500–3100 B.C.), and produced a set of Hyksos queen’s jewellery placed in a cenotaph. The large quantity of painted pottery imported from northern countries is thus dated, and gives a view of the high civilizations there over the palace period. The British School of Egyptian Archaeology (University College, London) will endeavour to find the temples this winter if the public will pay the workmen. Fifty pounds a week is needed to keep up the work. Even Irish archaeology has received a dating at 1500 B.C. by the gold work traded to Gaza. For historic archaeology there is no site with such a wide promise’.

Recent Events

The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.

We have been informed that the amended Ancient Monuments Act has just passed the Legislative Assembly of India and will be law in a short time. It enables English and foreign Universities and Societies to excavate in India. The terms on which they will be allowed to work will be much the same as those obtaining in Palestine and Iraq.



One of the most interesting exhibits at the recent Congress in London was that of flint implements from Kharga oasis, Egypt, by G. Caton-Thompson and E. W. Gardner. It is seldom that excavators can unearth such magnificent specimens of Acheulean and other older palaeolithic tools. Those exhibited were masterpieces of prehistoric skill. But of course the main value of the work done lies in the fact that it represents a definite, and very big, advance in our knowledge of the palaeolithic period in North Africa. The finds were not isolated but could be associated with definite geological horizons.



Mr Neville Jones has presented his magnificent collection of stone implements to the Rhodesian Museum, Bulawayo. Mr Jones is the author of a book *The Stone Age in Rhodesia** which, he says with refreshing but perhaps excessive candour, 'is now hopelessly out of date, and I do not advise anybody to read it'. That is the right scientific spirit, anyway! In a letter to us he states that he is 'anxious to build up a type collection representing the European culture-sequence; and I should like it to be known that I am open to negotiate exchanges'. (We have paraphrased his words). Will European museum curators please note? Here is a splendid opportunity of obtaining typical South African specimens for their own type-collections. Objects should be sent to Mr Jones at the Museum, Bulawayo, Rhodesia.

* Reviewed in *ANTIQUITY*, 1927, I, 114.

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Professor A. J. H. Goodwin has been excavating a cave at Mossel Bay in which he hopes to obtain evidence of the age of the coastal culture and in particular its relationship with the 60-foot raised beach. The cave was dug into some 30 years ago by Messrs J. Parrie and Gordon Leith, when a human skeleton and other bones were found. (*The Friend*, Bloemfontein, 14 July 1932).

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California has its giants as well as Dorset; but they are carved not in turf but in pebbles. They were discovered from the air and photographed (*Illustrated London News*, 10 September, pp. 378-80). Each of the two giants represented has a four-footed companion. They do not look very ancient. The published air-photographs show plainly a series of dis-tributaries covering the gravel plain and gradually advancing across it from the surrounding higher semi-desert; they seem to be of natural origin.

o o o

Yet other even more remarkable discoveries have been made, and photographed, by the Shippee-Johnson Peruvian Expedition, working in association with the American Geographical Society. They reveal a Great Wall, 30 miles long, a system of forts associated with it, and some finely preserved amphitheatres on the Maras Pampas, about 15 miles northwest of Cuzco. All these photographs are obliques. We hope that, in the interests of science, verticals also were secured. Obliques are not without value, but cannot compare with verticals for serious study.

o o o

One of the strangest things revealed by the camera is a band of pock-mark indentations running along a ridge, and also on its steep side. Professor Sir Flinders Petrie (who kindly drew attention to them) suggests that they may represent 'mining along a line of fault for ore; on the dark plateau on the right are many square chambers of the miners' settlement'. Perhaps one of our Peruvian readers will visit the site and tell us what is to be seen on the ground. We shall be pleased to send him our cutting from the *Illustrated London News*.

o o o

Mr Herbert W. Krieger, a curator of ethnology in the Smithsonian Institution, is proceeding to the Bahamas, to try and identify the 'naked, painted Indians, with fine high foreheads' who were the

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first inhabitants of the New World encountered by Columbus. He will follow up this investigation by exploring the shell-heaps of Cuba, which have never yet been scientifically examined. (*Nature*, 12 March).



An account of the fossil man found in 1927 at Asselar, Cercle de Kidal, French Sudan, has been published by MM. Boule and Vallois in Num. 9 of the *Inst. de Paléontologie humaine*. The skull and face are well preserved, cephalic index 70.9, cranial capacity about 1520 c.c. It resembles the Hottentot and Bantu types, and it is suggested that it and Grimaldi man are derivatives, perhaps associated with the Capsian industry, of an earlier less specialized type which divided, one going north and becoming Cro-Magnon man, the other going south and becoming ancestral to the Bushmen and Hottentots. (*Nature*, 20 August).



During a recent visit to Armenia the Editor was given a photograph of rock-engravings near Lake Gokcha (Sevang). For this he is indebted to the Archaeological Committee of Armenia. Though good it is unsuitable for reproduction, as the figures are so small that they would be invisible on a half-tone plate. It was submitted to the Abbé Breuil who reports that it shows 'six or seven Ibexes with very big horns'. He does not regard the style as very old, or of palaeolithic age, but rather akin to paintings on eneolithic pottery from the lower levels at Susa and Tepé Moussian. The relative nearness of Armenia to this region gives some support to this comparison. The technique is one of pecking with a hard stone.



An attempt has been made to locate the settlement belonging to the cemetery at Baldock, Herts, (see *Arch. Journ.* 1931, p. 246). The first trenches revealed skeleton burials, and later ones remains of dwellings (evidenced by post-holes and cobble pavements). At least three periods were detected; the second began apparently not before the second century; the third yielded a fourth century coin but may have begun as early as the third century. Other dwellings were found further east. The work is being carried out by the Letchworth Museum Excavation Committee, under the direction of Mr E. S. Applebaum.

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Mr Mansfield D. Forbes and Prof. V. Gordon Childe carried out a trial excavation in September at Old Keith, a ruined stone circle of the recumbent stone type near Alford in Aberdeenshire. Many potsherds of Early Iron Age type (not Romano-Caledonian) were discovered, some in such positions that they can hardly be posterior to the erection of the monument. Hence this type of circle is probably a good deal later than has been generally supposed.



A Committee for the Investigation of the Early History of the Fenland has recently been formed at Cambridge under the distinguished presidency of Dr Seward, Master of Downing. The region included in the terms of reference embraces the whole area of the Fenland past and present. The investigation is designed to cover the widest possible interpretation of history, including in its scope geological, botanical, zoological and climatological change in addition to the succession of human cultures in the area. The committee consists of working specialists strictly limited in number and carries no body of members. The co-operation of the Cambridge Antiquarian Society and the Prehistoric Society of East Anglia has been obtained, each supplying delegates to serve on the committee. The committee, realizing that in the past much valuable evidence has been lost through the absence of any central body for the co-ordination of these studies, will welcome any information on the subject; furthermore they will be only too glad to visit the sites of chance discoveries on the information of those on the spot. Anyone wishing to communicate with the committee should do so through the secretary, Mr J. G. D. Clark, at Peterhouse, Cambridge, who will put them into communication with the appropriate member.



The Athens correspondent of *The Times* reports an interesting discovery made in clearing out a well in the American zone of the Agora excavations. Three sherds (*ostraka*) were found bearing the names of Aristides, Themistocles, and Hipparchus, and appear to be undoubted relics of the ostracism of Aristides 'the just'. (*The Times*, 29 August, p. 10).



An account of the excavations at Tipaza, a small village and seaport some 60 miles west of Algiers, is printed in *The Times* (1 September,

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p. 9). The Service des Antiquités has begun work at the necropolis near the second-third century basilica and expects to find a large number of well-preserved tombs.



Finds of great importance have been made at Damgan, North Persia, where a joint Expedition of the University Museum of Pennsylvania and the American Institute for Persian Art and Archaeology is at work under the direction of Dr Erich Schmidt. Some fine examples of metalwork in copper, silver and gold, have been recovered from a building at Tepe Hissar. Dr Schmidt dates the lower levels of the workings as very close to the 4th millennium B.C. (*The Times*, 12 September, p. 11).



The Hebrew University in Jerusalem has decided to found a Chair of the Art and Archaeology of the Near East, which has been made possible through the generosity of Sir David Percival. Dr L. A. Mayer, of the Palestine Government Department of Antiquities, has been appointed as the first occupant. (*The Times*, 18 August, p. 9; 7 October, p. 11).



During the past summer a Harvard University Archaeological Expedition has been excavating a crannog or lake dwelling, of the Viking period, at Ballinderry, near Moate, co. Westmeath, Ireland, and an Early Bronze Age cairn at Knockast. The work has been directed by Dr H. O'Neill Hencken in association with Dr Adolf Mahr. A very interesting collection of articles has been recovered, including animal remains, stone querns, the coulter of a plough, wooden vessels and iron and bronze objects. The most important finds are a bronze hanging-lamp, suspended by animal heads and with elaborate patternwork on the bottom, and a wooden gaming-board, 9½ inches square, which is considered to be the finest object of the Viking period ever found in Ireland. (*The Times*, 7 October, p. 9 and illustration).



A report on the results of the excavations at Verulamium during this summer is printed in *The Times*, 24 September, p. 8. The western gate of the city-wall, by which the road from the Forum left in the direction of Silchester, has been found, and also an interval tower of second-century build.

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The excavations at Herculaneum which have been in progress for the past five years are being continued and Professor Maiuri, Superintendent of Italian Antiquities, describes in a special article contributed to *The Times*, 29 September, p. 11, the most recent discoveries, which are of particular interest in regard to the social life of the city.



Mr W. J. Maxton, Hayling Island Field Club points out that a statement in the review of *The Romans in Britain* on p. 372 of the September number of *ANTIQUITY* needs correction. Mr Richmond wrote that the Chichester inscription, stated in the book to be *from* Goodwood Park is *in* it, but Mr Maxton says that the 'Roman inscription now on the wall in North Street was *formerly* in Goodwood whither it had been taken after being dug up in Chichester quite close to its present position'.



The 'Grotto of the Sibyl' at Cumae, which has been the object of considerable investigation by Professor Maiuri, of Rome, has now been proved to his satisfaction to be the actual cave in which the foundation of Rome was predicted to Aeneas. A corridor nearly 700 feet long leads to the chamber which he identifies as the one where the Sibyl lived, and he considers that the description given in Vergil supports this without doubt. Professor Maiuri has published his account of the discovery in the *Bulletin of Mediterranean Studies* with very full details of the corridor and the galleries opening from it. These are given at some length in *The Times*, 13 October, p. 13, and 15 October, p. 9.



Mr Miles C. Burkitt, joint compiler with Prof. V. Gordon Childe of the Chronological Table of Prehistory which was published with the June number of *ANTIQUITY*, and which has been received with a chorus of approval, asks us to state that the word 'Mindel' at the top of column 29 is too high. Being of Tertiary Age the eoliths are all pre-Mindel. We may mention that copies of the reprint of the Table, with the introduction, can still be obtained (price 2s 6d) from the Assistant-Editor, 24 Parkend Road, Gloucester.

Some Recent Articles

This list is not exhaustive but may be found convenient as a record of papers on subjects which are within the scope of ANTIQUITY. Books are occasionally included.

A Handbook of the Prehistoric Archaeology of Britain ; issued in connexion with the First International Congress of Prehistoric and Protohistoric Sciences held in London, August 1-6, 1932 ; by various authors. *Oxford University Press*, 1932. pp. viii, 75, illus. 3s 6d.

Archaeology in England and Wales, 1914-1931, by T. D. Kendrick and C. F. C. Hawkes. *Methuen*, 1932. 18s.

We cite these two publications here and now, although we shall deal with them again later, because they fill a long-felt need. The first does not pretend to be 'a manual of British prehistory' (though it seems a good substitute), and the second has time-limits imposed ; but anyone who wants a good text-book (if we must use the word) cannot do better than buy both.

The Personality of Britain : its influence on inhabitant and invader in prehistoric and early historic times, by Cyril Fox, Director of the National Museum of Wales, Cardiff (by whom it is published) : 1932. pp. 84. 2s 6d.

To the two preceding items this monograph must be added. It is equally indispensable, lavishly illustrated, and admirably conceived. We shall review it at length later.

La Revue de Liban : les grandioses Ruines de Baalbek. Numéro spécial. *Paris, Geuthner*, 1931. pp. 24. 6 frs.

A popular tourist's 'Souvenir' of these remarkable ruins. There are numerous photographs and the letter-text is mainly a collection of passages from famous French authors who have visited, and written of, the marvels of Baalbek. M. René Dussaud provides a short and useful introduction W.J.P.-A.

Sulla posizione evolutiva di alcuni crani preistorici dell' Inghilterra : L. W. G. Malcolm. *Archivio per l'antropologia e la etnologia*, 1930-31, vol. LX-LXI, with separate pagination.

The Royal Anthropological Institute's Prehistoric Research Expedition to Kharga Oasis, Egypt : the second season's discoveries [1931-2]. *Man*, 1932, no. 158.

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Prehistory in Palestine: a Bibliography, by M. Stekelis. *Jerusalem*, 1932. pp. 41. No price stated.

Reprinted from 'Kirjath Sepher', vols. 8 and 9. The compiler of this very useful bibliography explains in his preface (which is also printed in Hebrew at 'our' end of the book) that it had been intended in the first place as preparatory work for an archaeological map of the prehistoric period in Palestine. Lack of the necessary means has unfortunately postponed its completion and the bibliographical material is therefore now published 'in its own right'. The compilation bears every sign of much painstaking and devoted work, and M. Stekelis' fellow-students will have cause to thank him for this lightening of their own labours. W.J.P.-A.

The Late Palaeolithic inhabitants of Palestine, by Sir Arthur Keith: substance of a paper read at the International Congress, London, August 1932. *Nature*, 20 August 1932.

Die Ausgrabungen der Notgemeinschaft der Deutschen Wissenschaft in Uruk (Warka) 1931-2: A. Nöldeke. [Bibliographical information obscure: separate pagination].

This valuable and well-illustrated report contains a good plan of the ziggurat and its surrounding temples.

Journal of the Manchester Egyptian and Oriental Society, no. xvii, 1932. *Manchester University Press*. pp. 68. 7s 6d.

The present volume is markedly 'monarchical' in character. Of the four special articles, three deal with 'Kingship', human and divine. Mr G. D. Hornblower writes on 'Temples and Kings in Ancient Egypt', Prof. M. A. Canney on the 'Magic of Kings', and Rabbi A. E. Silverstone on 'God as King'. No deliberate connexion is apparent between these, so that the coincidence is presumably accidental. There does, however, seem to be a distinct tendency (it is very evident in Prof. Canney's paper) to trace every ancient tradition, even to the story of the Walls of Jericho, to some cult ritual in which the king took the leading part. Prof. Canney deprecates our taking the Old Testament too seriously, and advances his own explanation of this 'myth' in the face of Prof. Garstang's very plain evidence of an earthquake! But perhaps Prof. Canney is not being 'too serious' about himself!

A murmur of protest ought to be raised against the price of this Journal. Surely it compares very unfavourably with that of other archaeological publications, for example *ANTIQUITY*. W.J.P.-A.

The Age of Pithecanthropus, by L. J. C. van Es. *Martinus Nijhoff, The Hague*, 1931. pp. xii and 142, with illus. and maps. 8 gld.

A geological study. The author concludes that Pithecanthropus is of Early Pleistocene age.

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Early Pottery on the Yorkshire Wolds [by T. Sheppard]. *Trans. East Riding Antiq. Soc.*, 1932, XXVII, 167-72, 6 figs.

These notes draw attention to some of the neolithic pottery of the Windmill Hill class in the Mortimer Collection, illustrated by excellent new drawings of the famous hanging Grimston bowls and of a hitherto unpublished little vessel from Esh's barrow near Cowlam. Mortimer's rather unsatisfactory sketches of the Towthorpe bowls are also reproduced, and welcome news is given of the identification in the collection of neolithic sherds from five new sites. It is to be hoped that these will soon be fully published. The last figure is given no provenance, but is actually another recently restored bowl from the Grimston barrow. There is no scale to any of the illustrations (they appear to be about $\frac{1}{2}$), but from them it can be seen that no. 248d is not 'of the same measurements' as 248c as stated in the text. S.P.

False and imitation Roman coins : some notes on moulds found, and coins manufactured elsewhere than in the imperial mints, by A. E. Robinson. *Journal of the Antiquarian Association of the British Isles*, Dec. 1931, II, 97-112.

The Towednack gold hoard, by Christopher Hawkes. *Man*, 1932, no. 222.

The Bed-canopy of the Mother of Cheops, by G. A. Reisner. *Bulletin of the Museum of Fine Arts, Boston*, August 1932, xxx, no. 180, 56-60.

A marvel of reconstruction, replete with technical details relating to early skill in carpentering.

Bulletin of the American Schools of Oriental Research, April 1932, no. 46 : issued quarterly to all contributors and subscribers of one dollar and upwards. Application for copies to Prof. Mary I. Hussey, Mount Holyoke College, South Hadley, Mass. European agent, J. C. Hinrichs'sche Buchhandlung, Leipzig C.1.

We have given full bibliographical details of this Bulletin, edited by Professor Albright, because it is indispensable to all who wish to keep *au fait* with exploration in the Middle East. The present number contains reports from Prof. Speiser on his excavations at Tell Billah and Tepe Gawra and an article by the Editor on 'New Light on Early Canaanite language and literature'. The last is concerned with the discoveries at Ras Shamra (see ANTIQUITY, 1930, IV, 460-6).

The Museum Journal, volume 23, number 1. *University Museum, Philadelphia*. \$1.

In this number of the Philadelphia Museum Journal Mr J. D. Beazley gives an account of a battle-loutrophoros, which he dates at about 440 B.C. He describes the vase as the best preserved of the battle-loutrophoroi, most of the others being extant in fragments only. There are articles by Mr E. H. Dohan on vases from

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the Henry C. Lea collection, 'A Late Minoan Pyxis', 'A Lydian Imitation of a Laconian Vase', and vases from the Hegeman collection. Mr V. Muller writes on a portrait of the Late Roman Empire. The number is very fully illustrated with photographs and drawings. G.F.F.

Hellenistic Queens : a study of woman-power in Macedonia, Seleucid Syria, and Ptolemaic Egypt, by Grace Harriet Macurdy. *Oxford University Press*, 1932. pp. xvi, 249, with 12 plates. 18s.

In this volume, which is number 14 in the Johns Hopkins University Studies in Archaeology, Miss Macurdy investigates the political power possessed by women in the three chief Hellenistic dynasties, and supports the view that it was in Egypt that the 'emancipation of the queens' took place. The scope of the book is limited to matters which involve the lives and characters of the queens and their political position. The paucity of material in the case of some of these royal personages and the method of chronological treatment of the queens of the several dynasties inevitably produces in places a certain disconnectedness in the narrative. The facts however are assembled with scholarly care, and if the writer protests against the tendency of the male historians to judge the conduct of the women rulers in such matters as dynastic murder by a higher standard than that by which they assess the kings, she provides the material for an independent judgment. The book is attractively illustrated with a number of portrait-heads. G.F.F.

Atti e memorie della Societa Magna Grecia : 1929 and 1930. pp. 216 and 205. 160 *lire* (1929) ; 125 *lire* (1930).

These excellent reports by the recently founded Society for the investigation of the Greek cities of Magna Graecia and Sicily provide material for a reconstruction of the much-neglected Greek archaeology of the West. It has long been a reproach to Italian archaeology that so little had been done. That reproach can no longer be levelled. In these two volumes a full account is given of Signor Marconi's very fruitful excavations on the site of Himera in Sicily. His discovery of the extensive remains of a fine archaic temple decorated with a superb series of lion sima-spouts and some important smaller finds show that Himera still has much to be excavated. The other researches of the Society have revealed the walls of Leontini and have gathered some scraps of evidence which may help to identify the hitherto unfixed sites of Sybaris and Laos. But both these cities still lie hidden beneath deep accumulations and their identity and extent are still uncertain. S.C.

Reviews

REPORT ON THE EXCAVATION OF THE PREHISTORIC, ROMAN, AND POST-ROMAN SITE IN LYDNEY PARK, GLOUCESTERSHIRE. By R. E. M. WHEELER, D.LIT., F.S.A., and T. V. WHEELER, F.S.A. pp. VIII, 137, with 28 text-figures and 52 plates. (Reports of the Research Committee of the Society of Antiquaries of London, no. IX). Oxford: printed at the University Press. 7s 6d.

The series of Reports issued by the Research Committee of the Society of Antiquaries presents an almost ideal format for publishing major excavations in this country, and it is matter for congratulation to all concerned that it has been enabled to include those of 1928-9 in Lydney Park. The Romano-British precinct of the god Nodens, with its temple and other buildings, and above all its remarkable mosaics, has of course been known since Charles Bathurst's excavations of 1805, illustrated first in Lysons' stately plates and then in the careful recension published by Bathurst (the excavator's son) and King in 1879. But there can be no doubt of Lord Bledisloe's sound judgment in deciding to effect the renewal of his great-grandfather's work, justified as it has been by the modern revolution in the technique of archaeology, and none likewise of the wisdom of its committal to the auspices of the Society, and to the genius of the spade, the camera, and the pen wielded by Dr and Mrs Wheeler. A goodly share of the same genius has already been displayed by Mr D. A. Casey in his work on the twelfth century castle near by (*Antiquaries Journal*, XI, 240ff.), and now the publication of the main site fulfils all our expectations.

First of all, we have the earthworks of a prehistoric 'promontory fort', defending a settlement of an Early Iron Age culture similar, as might be expected, to that of Somerset across the Severn Sea. The rampart across the neck of the promontory is astutely divined to be the earliest, but the outstanding feature of the earthworks is their reconditioning in post-Roman times, recorded here in anticipation of a later chapter. Little could be gleaned of prehistoric dwellings, but a date in the first centuries B.C. and A.D. is indicated by the pottery and other material: there is a remarkable iron bowl with ox-head feet, and Mr Dunning contributes a useful note, with distribution-map, on La Tène II brooches, their rarity in Britain, and the relative lateness of the Lydney examples.

The coming of Roman rule effected no violent upheaval: the same sequestered barbarian occupation lasted on into the fourth century without much sign of any very profound Romanization. Studies of the scanty coins, pottery, and brooches make this clear. Dr Wheeler has particularly useful notes on penannular brooches, and on the imported 'Langton Down' brooch-type of the first century: his remarks are confirmed by the incidence at Colchester of this Gaulish form in normally native associations, in contrast to the German brooches that arrived there with the Roman army. As late as the end of the third century, two huts, built on a levelled portion of the old rampart, were still un-Romanized and primitive. But one of them did the excavators more service than emphasizing this point. Its floor was found to seal the adit of an iron-mine—the first Roman working of its kind to be attested in Britain, and evidently not the only

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one on the site, which indeed is in part covered with such filled-in shafts. This one, being evidently of third century date, seems to belong to a later Roman expansion of an industry doubtless coeval with the prehistoric occupation.

The first five centuries of the site's history are thus instructive indeed, but undramatic. The change that befell it in the years 364-7 is positively theatrical.

The god Nodens, with his temple and all its appendages, appears on the site unheralded and unexplained. His name (on which Prof. J. R. Tolkien writes with learning and skill) appears to be Goidelic, indicating the same divinity that became in Irish tradition Nuada of the Silver Hand; its root should connote 'catching', but as well as being a hunter and a fisherman he was worshipped apparently as a sun-god and certainly as a god of healing—an intriguing example of the syncretism of fourth-century mysticism working on a primitive cult. Search beneath the temple for an earlier, perhaps a prehistoric, shrine was unrewarded by definite evidence, and the transplanting of the cult from elsewhere remains an open, and to this reviewer at least an attractive possibility: the site was a safe one compared with the open sea-plain, where barbarian raids had already been a menace for many years before 364-7, when (as secure coin-evidence attests) the Lydney temple was built. Its structure, and the rebuilding that followed the collapse of one of the cella piers into an unsuspected swallow-hole, the basilical plan with its unique peripheral 'chapels', and the religious implications of the whole, are admirably set forth and illustrated. The adjoining *hospitium*, with its curious fore-hall, the baths, and the long *abaton*-building where suppliants for healing probably repaired, as at Epidauros, for their 'sacred sleep', are next similarly described in turn: all are contemporary, and the precinct-wall was built last to enclose them. The whole complex is then reviewed in a lucid Summary. Later, the mosaics receive the special treatment that they have too long deserved in vain—it is a surprising reflection that this is practically a pioneer study, and it whets our appetite for Mr Roger Hinks's forthcoming British Museum catalogue. Among the other material, votive objects naturally loom large, especially the well-known dog-figures (the most famous strikingly portrays the Irish wolf-hound); Mr Collingwood deals with the inscriptions, including the 'Lydney curse'; there is a notable series of pins and of bracelets, and brooches are of normal fourth-century types.

Throughout, the many earlier finds in Lord Bledisloe's possession are published together with the new, and the whole makes a remarkable picture of the life and wealth of the shrine in the last half-century of Roman Britain proper. In no field is this more interesting than that of the coins: here Mr J. W. E. Pearce has rendered signal service, especially in his study of Valentinian mint-marks and of the temple-hoard of Constantinian bronze, with its valuable evidence for contemporary barbarous copies of the FEL. TEMP. type. It is, moreover, this barbarous coinage that leads us on to the last phase of the history of the site, when the decay of the temple buildings and the reconditioning of the prehistoric earthworks made it a 'hill-fort' once more. The added rampart-material contained much broken fourth-century pottery, and the whole episode is clearly one of sub-Roman barbarism. A remarkable brooch based on a Gothic type reinforces the point, and it is amazingly illuminated by the now famous hoard of 1,646 tiny barbarous bronze coins found against the broken edge of a mosaic floor in the baths, sealed over by a later cement repair. Mrs Wheeler's skill and patience have given us a brilliant classification and an exhaustive but always readable description, fully and strikingly illustrated. It is remarkable that while official coinage as late as Honorius reached the site, these barbarous *minimi* and *minimissimi*, which must belong to the fifth

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if not the sixth century, are all based originally on the Constantine FEL. TEMP. design. The whole study makes a refreshing start on what should be a most vital approach to the rediscovery of our Dark Ages. The barbarous coinage is a by-way in numismatics no longer. Incidentally, the total absence of sub-Roman pottery is significant. The fourth-century wares are all regular mass-products : domestic potting in the fifth must here have been dead beyond recall. It is a warning against pressing such negative evidence elsewhere. It also puts a premium on positive evidence. With the Lydney Report in our hands, we shall follow Dr and Mrs Wheeler all the more keenly at Verulamium.

CHRISTOPHER HAWKES.

INDEX OF POTTERS' STAMPS ON TERRA SIGILLATA, 'SAMIAN WARE'.

By FELIX OSWALD, D.SC., B.A., F.S.A., F.G.S. With a supplement. Hand-printed and published by the Author at Margidunum, East Bridgford, Notts, 1931. pp. xxiii, 428. 36s 9d post free from the Author.

It is hard to know whether Dr Oswald's learning or his patient determination calls for the higher praise from the student of Roman archaeology who has (and all must have) this book in his hands. It is nothing less than that reasoned catalogue of 'Samian' potters' stamps in all their variations, recording the sites and shapes of vessels on which they have been found, to which forty years of study have been looking forward. The task of compilation, in which Dr Oswald has been ably assisted by his son, has been huge—the aggregate runs to some 20,000 items—and that of presentation no less so. For no publisher would undertake a work of such restricted appeal, and so the author set about its reproduction himself. The duplicator which was his first expedient had to be given up and 120 pages and 300 copies scrapped when the process was found unsuitable, and the whole undertaking begun anew with a Roneo hand printing-press. With this Dr Oswald has worked for over three years, in what time he could spare from his official duties, setting up and breaking down the type for every page in turn. The impossibility of reproducing ligatures, aberrants, and retrograde forms save to a very limited extent, was of course inevitable, but while we await his hoped-for album of facsimiles, we cannot stint our prizing of what Dr Oswald has here given us.

The volume is, for its size, pleasant in form : the text is as clear as could be wished, and typographical errors are almost wholly absent. With its strictly alphabetical arrangement, and thoughtfully designed cross-references, it is as easy to consult as could be imagined. And to every museum-curator, excavator, and student its consultation is indispensable.

Great Britain, France, Spain, Italy, Switzerland, Austria, Germany, Holland, and Belgium have all been laid under contribution, and while Dr Oswald acknowledges help from many correspondents, an enormous number of the entries have been obtained by personal visits by himself and his son to inspect originals, wherever housed.

The preface lucidly explains the method of presentation, and takes more than a glance at many of the questions implicit in the names (ethnology, partnership, orthography), in the provenances (spread and distribution of potteries and markets served), and in dating. In the latter regard we have (p. xv), a select list of dated sites of restricted occupation, while individual potters are dated as a rule by the reigns of emperors or dynasties only—a wise precaution, for even in the list of sites some of the dating-figures look somewhat over-exact (and the earth-fort of A.D. 47 at Caerleon has as yet no more than an ideal existence).

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A list of dated potteries follows : here again one or two limiting dates seem a little harsh, *e.g.*, Lubié terminating 160, despite the dating (p. xvii) to 169-180 of the Pan Rock find which included *e.g.* CALETVS, MAINACNVS, MARTINVS, PAVLLVS. Similarly St. Bonnet cannot have ceased work in 130 (*e.g.* CINTVSMVS at Pan Rock) ; perhaps 180 should be read. Errors in the body of the work are as few as omissions : BELSA ARVERNVS rather than ARVERNICVS is perhaps preferable ; the queer occurrence of OF CAI from Pan Rock in the British Museum is unnoticed ; it is not the $\alpha\delta$ monogram, but the full Lezoux stamp DOHCVS, that occurs at Lowbury Hill ; the first N of MATERNIANVS of Lezoux is occasionally doubled ; MINNA is surely ANNI M retrograde. External stamps 'in the field' on decorated bowls might have been specified. But these are trifles. The book is a magnificent piece of work, and Dr Oswald is probably the only man who, under such conditions, could have produced it. He has indeed deserved well of the republic of archaeology.

CHRISTOPHER HAWKES.

PRÉHISTOIRE. Edited by RAYMOND LANTIER, Conservateur-adjoint, Musée des Antiquités Nationales, Château de St. Germain-en-Laye, Seine-et-Oise, France. Tome I, fascicule 1. *Paris : Librairie Ernest Leroux, 28 rue Bonaparte. pp. 123, with 5 plates in collotype and numerous illustrations in the text. Annual subscription to volume of two or more fascicules, 250 francs (price of separate fascicules varies).*

M. Raymond Lantier is well known as one of the ablest successors of his great countryman Déchelette : a much travelled scholar of European outlook, who occupies a place of vital importance—it was till lately Henri Hubert's—in the French National Museum at St. Germain.

His foundation of a new periodical has been eagerly discussed, and the first number of PRÉHISTOIRE will be as eagerly scanned, no less for the admirable articles which it contains than for the Editor's statement of policy with which it opens. M. Lantier lucidly explains his purpose. His field is the whole range of 'problèmes relatifs aux origines, aux développements, et aux rapports des civilisations du vieux monde depuis les temps les plus reculés jusqu'aux époques des grands empires historiques'. On it there browse already an innumerable flock of Journals, Proceedings, Transactions, Bulletins, Revues, Mémoires, Cahiers, Zeitschriften, Anzeiger, Korrespondenzblätter, Annalen, Vorgeschichtsfreunde, Notizie degli Scavi, and many more, in all languages. The great majority are the organs of local or regional archaeological societies ; others are of national societies or institutions ; a very few are, like ANTIQUITY, independent. In form, scope, and scientific standard they exhibit infinite variety. But for the short article, excavation-report, museum-study, or archaeological note, they provide abundant—often fatally abundant—facilities. For studies which are not so much news-items as fully-fledged essays or monographs, publication is normally more difficult. Such works often run to some length ; they demand a high standard in illustration, and they must be of recognizably permanent value if they are to deserve publishing at all. The journal of a local society is seldom the place for them : in one of national scope they may block the way of many articles important for their current news value. Some societies solve this problem by maintaining two publications, a quarterly or monthly journal and a usually annual volume of weightier matter, as our own Society of Antiquaries does with the ANTIQUARIES JOURNAL and ARCHAEOLOGIA. But few societies can afford this, and in any case there is a real dearth of publications of either kind of truly international range and independent management. ANTIQUITY is almost unique in these respects among

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journals that are real reviews, publishing material which, whether from the field or the study, is archaeological *netos*. What M. Lantier is aiming at is an equally independent vehicle for 'exposés systématiques' and 'perspectives synthétiques', including news articles only in outstanding cases of general interest.

It is a well judged and skilfully planned venture. The names of the Editorial Committee are worth giving: Bégouen, Bersu, Bosch Gimpera, Breuil, Burkitt, Constantini, van Giffen, Mauss, Menghin, Obermaier. M. Lantier is the executive chief. Each annual volume of some 240 pages will be issued during the year in two or more conveniently sized parts. The yearly subscription is 250 francs, or £2 at par. The page is a good broad one, an inch and a half broader than *ANTIQUITY*, and nearly as much taller—about the same size as the new *MAN*. The type is a pleasant French face. The paper is strong and shiny, takes line-blocks well, and is also suitable for half-tones, which accordingly appear in the text, and are as good as most half-tones to which fine art paper is denied. But though this means that they are sometimes no more than fairly adequate, important full-page plates are in collotype on special paper. The results in this first number, though not in every case quite excellent, are most promising. For instance, the illustrations of the first article, an admirable study by Dr Henri Martin of the Solutrean sculptures of Le Roc, compare well with the half-tones of Dr Martin's earlier paper in *ANTIQUITY* (March 1929) III, 45ff: the battle of the two he-goats in relief is remarkable, and the engraved bird is a notable rarity in palaeolithic art. Prof. Obermaier next describes a series of *bâtons de commandement*, harpoons, and other pieces of Late Magdalenian *art mobilier* from the Cueva del Pendo near Santander, which include an interesting fish-pendant, and well illustrate the schematizing tendencies of the period.

The rest of the number is taken up by a masterly study by Dr Robert Forrer of cult-chariots and wheeled vessels throughout the range of their prehistoric varieties, tracing their survivals in various parts of the world into historic and even present times. It is a fine piece of synthesis, bringing together much earlier work (of which Déchelette's is perhaps the best known) on the solar religion of prehistoric Europe and its symbolism, together with comparative material tirelessly collected from all over the world. Nor is it only a study in *Religionsgeschichte*: Dr Forrer is led to a most compelling theory of the origin of wheeled vehicles in general, the secular no less than the cult-chariot being derived from the sun-disc devised by the solar worshippers of the earliest European Metal Age. His imposing mass of material is illustrated in 38 plates, among which the four views in collotype of the famous Iberian chariot of Mérida—Dr Forrer's own gift to the St. Germain Museum—are very properly outstanding.

The appetizing advance list of papers promised shows that Western and Central Europe is to be the main sphere of *PRÉHISTOIRE*, and French the normal language of its contents, but M. Lantier need not be suspected of meditating any formal restrictions on the full scope of his choice. All readers of *ANTIQUITY* will wish him well, and we hope that even in these times English subscribers to *PRÉHISTOIRE* will form a goodly proportion of an ever-lengthening roll.

CHRISTOPHER HAWKES.

MAP OF NEOLITHIC WESSEX, published by the Ordnance Survey, Southampton, 1932. *With 35 pages of text and 2 figs.* 4s.

This map is published by the Ordnance Survey in pursuance of its policy of surveying the megalithic remains of Britain according to the quarter-inch maps. Sheets 8 and 12, which have already been published, were dealt with in *Professional*

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Papers, but in the present case (sheet 11) the great wealth of material justifies its publication as a map with subsidiary text and list of sites. The result, it will be generally admitted, does great credit to the Ordnance Survey, and in particular to its Archaeology Officer and his voluntary helpers.

The map itself is a thing of beauty as well as an instructive summary of field-work, and is fully in keeping with the artistic traditions associated with the publications of our National Survey. It is printed in five colours, the contours being layered in four shades of brown, every 200 feet. The forest areas have been reconstructed on a geological basis and are shown in green. Similarly the former marshes and the present rivers and estuaries are shown in blue, the sea-coast being shown as at present, owing to lack of data for reconstructing the neolithic coast-line. Behind all this vivid landscape the pale grey ghost of modern England shows through—unnoticed except when wanted for reference—the reverse of the more familiar relationship of things ancient and modern.

There are 187 neolithic sites marked, and these comprise long barrows, burial chambers (formerly known as dolmens), circles of stone, earth and wood, habitation-sites (including causewayed camps), inhabited caves and flint-mines. These are indicated by appropriate symbols in black, the whole showing at a glance the ecology of neolithic man (if one may borrow a term from the botanists) better than a bookful of description.

The inclusion of certain sites, notably circles of stone, earth and wood, of which the 'neolithicity' is not beyond question, is admitted in the text. This is a result chiefly of the difficulty of defining the limits of the neolithic period, and partly of the fact that the survey was primarily megalithic, which is perhaps rather an inexact classification seeing that megalithic remains were not all necessarily of one period. But the principal difficulty arises from the overlap of the neolithic with the beaker period—assuming that Peterborough pottery is regarded as neolithic and that beakers are relegated to the Bronze Age, even though the actual metal may not have appeared with the first arrivals. If Peterborough pottery is found in one circle, for instance, then circles as a class must be regarded as potentially neolithic, even if they may be more characteristic of the frank Bronze Age, and so all circles must be placed upon the neolithic map lest any truly neolithic examples be omitted. The non-appearance of such apparent, though unproved, causewayed camps as Scratchbury (inner ring) and Rybury (outer ring) is a touch of caution in the opposite direction, doubtless inspired by the failure of the inner ring at Yarnbury to fulfil expectations.*

Some consideration, illustrated by two plans, is devoted to discovering the routes by which the neolithic folk of the chalk plateau of Wiltshire may have communicated with their neighbours of the Cotswolds and the Mendips across the belt of clay-forest which separates them. Satisfactory evidence is given for locating the crossings at two points, near Lacock and near Frome, respectively.

A minor shock awaits the precisionist when he finds that the first two long barrows listed as being in 'Wessex' are actually in Glamorganshire. This is, of course, because the 'Wessex' in question is not just the area of the Saxon kingdom, but that of the quarter-inch O.S. sheet which covers that kingdom and a good deal more besides,

* See note by Mrs Cunningham, p. 471 *ante*.—EDITOR.

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including a corner of Wales. It is excusable in such a case that precision should yield to expediency.

On p. 13 'fig. 2' should read 'fig. 1' (twice), and in the footnote on p. 27 there is a minor error of type.

It is only by such distribution-maps as these that bygone races can be studied in relation to their geographical environment. It means years of painstaking teamwork among voluntary workers, but its results are abundantly worth while, and this publication is yet one more addition to the ever-growing list of books that are indispensable to the modern student of prehistory.

E. CECIL CURWEN.

THE GEOGRAPHY OF THE MEDITERRANEAN REGION, ITS RELATION TO HISTORY. By ELLEN CHURCHILL SEMPLE. *Constable*, 1931. pp. x, 737, frontispiece and 14 maps. 21s.

This noteworthy book is the result of many years' work and fills a real gap in our literature better than most authors could fill it. One is impressed not by how much geography Miss Semple knows, but how much ancient evidence she has collected in order to illustrate the intimate connexion of her science with ancient history. Not all her information has been gathered at first hand from the authors, and this, perhaps, is hardly to be expected. Again, she concentrates rather upon Greece and the Levant than upon Rome and the western provinces; but that is not to minimize the amount of useful information which Roman students will find and appreciate for themselves in her book, even if they wish that the balance had been held more evenly. Nevertheless, it is worth pointing out that to the ancient historian the geography of Greece and its effects are tolerably well known, while no adequate account of ancient Spain exists. To this extent, an opportunity has been missed.

The opening chapter explains briefly and effectively why the study of the Mediterranean matters. Not only do we owe an immense amount to the cultures evolved there, but geography compelled man to shape them in certain definite ways; how definite we hardly realize until Miss Semple has told us. One would like to hear more of her views upon how it influenced men's thought, for these peep out only rarely, in connexion with geology (p. 35), cataclysms (55), and gods (caps. XVIII and XXI). But an interesting subsection could be written, which would add to the value of a second edition.

The book then deals with the general geographic conditions, the barrier boundaries, vegetation and agriculture and maritime activities. The whole treatment is notable for its clarity, and is packed with information, supplied with enviable ease and economy of diction. Ancient historians will read it with gratitude and pleasure. Among the more striking comparisons we would note the frequent reference in agricultural matters to the Japanese, whose unremitting labour provides the norm for the amount of work put into intensive cultivation in ancient times. A useful observation is the comparison of Rome's early activities in the south of Italy and the Ionian sea with those of the Norman kings (76); while historians will welcome her conclusion (100), reinforced by rainfall statistics (506-7), that there is no fundamental difference in climatic conditions between now and ancient times. The diagnosis of Egypt's weakness (157) is neat, and prepares the way for the really brilliant sketch of conditions governing the occupation of Syria. But it is much to be regretted that the treatment was not carried

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forward into Roman times, during which the treatment of the Euphrates frontier is only explicable upon a geographical basis, and really illumined by the rainfall-map (192), which, however, marks few of the places mentioned in the text. Conversely, map 10 (220), showing the ancient fortifications of the Peartree Pass, makes us long for the text that military exigencies forbid, and moves us to observe that, since excellent studies of antiquities inaccessible to civilians have in the past been published in the Italian *Rivista d'Artiglieria e di Genio*, an examination of these works might enliven the service and improve the efficiency of any young officer. Meanwhile, this is probably the best account of the fortifications that we shall have. After this, Miss Semple's description of Narbonensis, happily defined as 'a geographical handicap . . . converted into a geographical opportunity', and the explanation of the development of the province into Burgundy are worth special attention (248 ff).

The chapter on stock-raising is particularly valuable. One is indebted to the author for illustrating the important part played by the Veneti in horse-breeding (pp. 308, 326), and for her recognition of Polycrates as an all-round improver of stock (309). That versatile tyrant seems to have left few activities untried. In dealing with agriculture in general the discussion of Sparta's grain-resources (349-50) is matched in interest by information as to the proportion of barley to wheat in Attica (351) and as to the weight of that wheat compared with the Boeotian (353); this should be compared in turn with the statistics of general Mediterranean yields (388) for a realization of the poverty of Attic soil. It should also be noted that the author groups the factors which governed the creation of Greek colonies in the following order of importance, namely, food, politics, and commerce (689, cf. also 354). This is the order imposed by natural conditions. Incidentally, these same natural conditions may have brought about dwarfing in stock (324). Interesting also are the remarks upon Rome's early colony-allotments and their size (426-7), especially in relation to the ideal of intensive farming, in which respect we should also note the tribute paid by the author to both Greek and Roman practical knowledge of facts whose cause is only now explained by science (395-6, 407, 420).

Criticism of a book of this type and of such learning is necessarily confined in the main to asking for more. But we would note some cases of failure to reconcile divergent evidence and to collate relevant facts. The evidence for earthquakes in Italy of p. 39 should be reconciled with that of p. 50. There are other ports of Latium (548) than Formiae (595). A bridge is implied at Tarascon on p. 114, while a ferry is correctly mentioned on p. 553. The list of towers as landmarks (591) ought to have included the tower of Caepio (558); but it is interesting as showing how the lighthouse of antiquity was not a danger-signal, but a guide into port. The best-known danger mark, near Sciathus, is of Persian origin (Herodotus, VII, 182) and might have been mentioned. The variant widths of the strait at Rhium (37) are not impressive evidence for geological change, if Scylax is put in his correct order. The change-over from herding to agriculture in Thrace (356) might be connected, on the principles of Professor Carr-Saunders, with its large human resources in Augustan times (318).

Some omissions and errors may also be noted. Why is the crystalline area of Tuscany omitted from the map (18)? The equation Lilybaeum-Palermo would help the modern reader (79). The ancient Pont du Gard (114) had no road-bridge. The exaggerated tale of Cornish tin (140) dies hard: but the evidence about it is not really inaccessible, any more than Haverfield's note on the Cassiterides. On the other hand, ancient exploration sometimes went further than it is given credit for having done:

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the expedition of Suetonius in Africa is a perfectly well-accredited case of trans-Atlantic exploration (cf. 145). We wonder why the Delphi charioteer was taken (177) as representative of eastern-style archaic sculpture. The special style of construction evolved in late-Roman Italy (290) to meet wood-shortage is a piece of unverified information. Why, again, should Rome's chief temple be transferred from the Tarquins to Augustus (533)? Finally, Roman Emperors were not crowned, and that was not why Vitellius returned to Rome (118).

Against this list, we would put on the credit side some odd scraps of information which show how wide Miss Semple's reading has been. The reason for the importance of Adalia (122), Byzantium (214), Aquileia (227), Heraclea Pontica (276) and Tarraco (116, whose harbour was better than the modern coast-line suggests) have rarely been better stated. Again, we welcome the many-storied houses of Tyre (42); emphyteusis applied to forest-clearing on Cyprus (272); the ploughing-spade of Thessaly (387); the sewage-farm at Athens (414); artichokes as wealth at Corduba (436); the rivalry of Mantinea and Tegea (450) and the origin of the Eurotas (445-6).

There is rather a heavy crop of misprints. Orenso (19), *incredible* (119), Ostium (128), Boné (128, 284), Mimms (131), Silvius Italus (133), *pied e terre* (144), rock-and-pinion (181), *irredenda* (237), Hyeros (244), Monoei (288), Hippias (306), four acres and a cow (426), Sheria (435), Volci (452), Cossa (452), Vetii (499), Nolscian (545), Caleman (547), Llobrega (555), Barcina (556), Sierra de Tolox (557), 313 B.C. (571), *militae* (229), Megarans (309), Coae (679), Serapheum (705). Port of Augustus is not the modern form of Portus. The book would be greatly improved by the provision of a subject index.

I. A. RICHMOND.

LE MURA DI ROMA REPUBBLICANA; SAGGIO DI ARCHEOLOGIA ROMANA. *Di* GOESTA SAEFLUND. Skrifter utgivna av Svenska Institutet i Rom, I (Acta Instituti Romani Regni Sveciae, I). Lund: Gleerup and London: Humphrey Milford, 1932. pp. xvi, 278, figs. 1-72, plates 1-27, plan. 25 kr.

The modest title of this book will convey little to those unacquainted with the remains of the Wall which form its theme. These amount to a few disconnected scraps, each one sufficiently unlike the other to make it uncertain from the evidence on the spot whether they are typical examples of what has disappeared. The earth-bank, which was an integral part of the system in certain sectors, is nowhere visible now; and those who studied it, when it was removed to make room for the present Railway Station, recorded two periods, but preserved none of the evidence for dating them, to secure which would be the first object of any modern inquiry. Add to this a meagre series of laconic literary sources and a set of sketchy but valuable notes taken by Lanciani, and the sum total of the evidence is before us. It is, then, surprising that this material should not have been thoroughly surveyed before, and it illustrates the late Dr Ashby's remark 'that the monuments of the power of Rome in the remotest borders of her Empire are better known than those that lie at her very gates'. Actually, the ancient sources about this Wall had been studied in great detail, but the existing remains had not been exhaustively described as a whole, while Lanciani's notes, made during discoveries and demolitions in different sectors, were not available until presented to the Vatican in 1925. Thus, Dr Saeftlund unites this material for the first time, and may justly claim to have placed the historical aspect of the whole subject upon a sound basis by his thorough work.

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His principal thesis, that the original wall of Rome, assigned by tradition and by many modern authorities to Servius Tullius (578-534 B.C.), is of fourth-century date, has been advanced before. But never has it been supported by so strong a chain of evidence. For Dr Saeftund discusses in detail each sector whose construction has been dated to the sixth century, and finds in each case that elements of much later date are present at crucial points, though they had been neglected by those who did not look at the monument carefully enough. The thinly-coursed yellow-grey tufa masonry, hitherto taken for typically sixth-century work (on the basis of certain monuments whose dating is undisturbed by the present conclusions), now is proved to be late repairs, looking antique because an antique type of stone-cutting is imposed by the geological formation of the stone. The oldest work, then, turns out to be executed in Grotta Oscura tufa, drawn from quarries inaccessible to Rome until after the fall of Veii in 395 B.C. and signed with Hellenistic masons' marks, whose affinities Dr Saeftund is the first to recognize, having drawn up a complete table of the available evidence from the Mediterranean basin. Thus, the date imposed by the material nicely coincides with Livy, who dates the building of a wall to after 378 B.C., when Rome had recovered from the shock of the Gallic fire. The association of this great work with the Hellenistic cycle of builders is one of the most important conclusions reached.

Servius Tullius is left with the *agger*, which blocked the access to Rome from its northeast plateau, always the city's most vulnerable side. Elsewhere, steeply scarped cliffs offered a natural defence, whose contours have been largely obliterated by the modern city, and can best be visualized by looking at Veii or Ardea. The meagre evidence at our disposal fits the ascription of the first *agger* to the sixth century. It was an earth-mound, five metres high, formed of clean upcast from a ditch of corresponding dimensions. It is, however, uncertain when that earth-mound, being fronted in 378 by a wall at least eight metres high, received its heightening. The author delays the measure until the Wall was refurbished to ward off Hannibal in 212 B.C. But it is not illogical to assign it to the moment when the Wall came into being, and a side-light is thrown upon the quality of the evidence available when it is realized that either interpretation can be admitted.

The Wall owes its final form to Sulla, who, dealing with the Marian disorders of 87 B.C., equipped it with artillery. At the gates, where attack might always be anticipated, permanent casemates for spring-guns were provided, two of which remain, the well-known one on the Via di Porta S. Paolo and the other in the Palazzo Antonielli, whose identification we owe to Dr Saeftund. It is suggested that the *agger* was once more extended at the back, and that this enlargement of the base stands for a general doubling of the wall in height. But the extension itself is proved to go back to an epoch before Sulla by its association with the niche of the altar of Verminius, which was renovated in Sullan days; while the heightening lacks cogency as an interpretation of any extant remains. In date, the backward extension might be as old as the Hannibalic scare of 212 B.C., and the revetment thereof a stage later; and the explanation that a backward reinforcement of the mound was required if it was to serve as an artillery-platform, without any increase in height, is at least worth examination, for it is unnecessary to connect the first provision of these machines with Sulla. Dr Saeftund, in fact, is too inclined to telescope all these operations into one Sullan scheme, though decisive evidence on the point will hardly now be forthcoming.

It is hardly the place here to deal with the questions of topography and building-materials which are raised in some detail by Dr Saeftund, and demand a full

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discussion. But we may call attention to the chapter on analogous walls, which forms the basis for the following theory of evolution, as applied to Italian town-walls in general. The earliest fortifications were slight reinforcements of steeply scarped cliffs, supplemented by earthen ramparts where this was necessary, as at Rome and Ardea. The chief element was the citadel. Next followed the terrace-wall and its free-standing accompaniment, never very high, and often associated with an earth-bank. These remained the standard type until the fourth century, when the Hellenistic and Carthaginian development of siege-tactics began to demand higher and stronger defences. But, although new walls were built in accordance with this need, the adaptation of old ones followed sluggishly, probably more slowly and with more exceptions than here suggested. The third and second centuries saw the creation of walls in terraced series, and the building of some very high ones, among which Osimo might have been quoted. But the material on which to build these generalizations is rather scanty, and Dr Saeftlund fails to observe that the Sullan age seems to have gone back to the older standard of a low wall, well-protected by towers furnished with artillery, as at Fondi and Terracina. This point has some bearing upon the question whether the Rome wall was heightened in its latest period.

Among less general matters, it may be noted that triple and double gates are less rare than is assumed, and it is tempting, in view of the Hellenistic connexions of the builders of 375, to think that the great gate of the Euryalus (see *ANTIQUITY*, September, 1932, pp. 262-9) was the model for the Porta Trigemina. The date when the portcullis was introduced into Italy is uncertain, but its presence is not really well attested before the Sullan age, though one is prepared to carry the date back at any moment. It occurs, for example, in the second-century wall of Carmona, in Baetica. It is difficult to agree with the suggestion that the first wall at Ostia can be so late as the Hannibalic wars, in view of the town's association with the early maritime colonies, and we would draw attention here to the likeness between its plan and that of Pyrgi. Finally, round-topped merlons came nearer Italy than Gjölbaschi: they were used at Motya by the Carthaginians.

The book is well-illustrated, and the plates are beautifully drawn in a fashion that suits the subject well. We regret, however, that the text-figures were not larger, and, in some cases, better produced. But the book as a whole is a great credit to its author and to the Institute under whose auspices it is published. I. A. RICHMOND.

DISTRIBUTION OF THE ABORIGINAL POPULATION OF MICHIGAN.

By W. B. HINSDALE. Occasional Contributions from the Museum of Anthropology of the University of Michigan, no. 2. *Univ. of Michigan Press, Ann Arbor, Michigan*, 10 June 1932. pp. 35 and coloured map. Price not stated.

This admirable geographical study is based upon the data provided by the Archaeological Atlas of Michigan.* It consists of a detailed description and analysis of the distribution and relative density of aboriginal population in the State of Michigan. The writer's attitude is thoroughly sound, and he bases his conclusions upon the character of the aboriginal environment. He shows (and illustrates by a map) that the areas where deciduous, or mixed deciduous and coniferous, forest prevailed were those preferred by the aboriginal population; the reason being that here the food

* See *ANTIQUITY* June 1932, p. 231.

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supply, animal and vegetable, was most abundant. This natural environment is in its turn determined by the surface geology (p. 6). 'The man-land ratio, for people in the hunting-agricultural stage of culture, as were the Michigan Indians, depends upon the productivity or fertility of the soil in regard to plant and animal products available for and convertible into human food' (p. 8).

There are some interesting, and sometimes entertaining, sidelights upon the policy of capitalist exploitation which everywhere in the world has wrought such havoc. 'The Indians could not endure ruthless destruction of any of the objects of nature, animate or inanimate. They were the first and most scrupulous conservationists. They thought it improbable that Hell, as described to them by the missionaries, was a place where fire burned eternally because there would not only be a great waste of timber but, they argued, the woods could not last that long' (p. 21). This sensible concrete argument is irrefutable, and may be commended to the consideration of, say, the Catholic Evidence Society.

Another interesting sidelight on the economic basis of, in this instance primitive, warfare is cast by the statement that 'quarrels over the privileges of harvesting the wild rice in Green Bay and contiguous waters often broke out into open warfare among the Michigan and Wisconsin tribes' (p. 22).

But it is the demonstration of the environmental control of population density that forms the most valuable part of this monograph. It is backed up by figures. Those who are interested in prehistoric European distributions and in the similar control there exercised by environment will welcome Mr Hinsdale's analysis, which is based upon more easily ascertained facts. It is easier to restore the primitive vegetation and faunal distribution in a 'new' country like America, where it survived down to modern times. Documentary accounts of it supplement botanical observations that can still be made in surviving patches. But, whatever method we employ, and whether the task be relatively easy or (as in Europe) difficult, this is the way to study primitive man. Here is a branch of enquiry which will be followed with increasing frequency as data accumulates in sufficient quantity for mapping to be possible. For, until the data are numerous, the negative evidence (of blank areas on the map) may be misleading. We look forward to other and perhaps more elaborate studies on similar lines.

O.G.S.C.

LOCAL PAPERS, ARCHAEOLOGICAL AND TOPOGRAPHICAL—HAMPSHIRE, DORSET AND WILTSHIRE. By HEYWOOD SUMNER, F.S.A. *Chiswick Press, 1931. pp. 248. 12s 6d.*

Any recommendation of a book by Mr Sumner is superfluous. As archaeologist, artist and writer of fine prose he holds a unique position, and this volume of collected papers, reprinted from various societies' transactions, is a worthy successor and companion to his previous books both in archaeological content and charm of illustrations.

His account of the excavations of six barrows on Ibsley Common is of particular importance and interest. Barrow 1, an abnormal disc- or rather saucer-barrow, yielded a cinerary urn of middle Bronze Age form, which gives an indication of date for this type of barrow. The constructional details recorded in the other (bowl) barrows of the group are of all the greater importance since such features were almost universally ignored by the excavators of the last century, on whose work our knowledge of the middle Bronze Age has mainly to rest.

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A survey of Comb's Ditch, in Dorset, and a comparison with its neighbour, Bokerly Dyke, is a valuable contribution to the study of these linear earthworks. The student of minor earthworks will find puzzling and mysterious examples described and planned—the Bee Garden; the earthwork at Cowgrove, for which a Saxon origin is suggested; and the Fairy Ring in Pinnick, prosaically but convincingly explained in picture and print as a medieval pig-pound.

A reprint of Norden's MS survey of 'Copices in New Foreste', 1609, is an attractive excursus, and forms a link between the archaeological and topographical papers in the book. It is to these latter that the archaeologist, his scientific thirst assuaged, will turn a second time, and with Mr Sumner as a perfect guide will once more explore the Latchmore Brook from its source to the Avon, examine hill-top ponds in flood and drought, or meditate on venerable trees and the passage of years.

Mr Sumner's plans are, as always, both beautiful and interesting. Although unconnected with archaeology one cannot resist mention of the very attractive map opposite p. 215, with its deliciously wicked vignette of picnickers ignoring a cautionary rhyme.

STUART PIGGOTT.

TEXT-BOOK OF HISTORY FOR THE 5TH YEAR GYMNASIUM AND 1ST YEAR TEKNIKUM. By O. V. TRACHTENBERG, A. E. GUKOBSKY and V. N. BERNADSKY. *State Elementary-Pedagogical Publishers, Moscow and Leningrad*, 1931. pp. 183, illus. 70 kopeks.

Universal history is not yet taught in British schools as part of the established curriculum—or indeed at all. When therefore I came upon a classroom of children in Tiflis who were learning Universal History it seemed worth while enquiring further into such an interesting phenomenon. The text-book they were using was on sale in the town; I acquired a copy, and went through it with my Russian companion who translated the essential portions. I cannot claim to have read the book, of course, but merely to have been enabled to grasp the principles of its arrangement. It is intended for children of about 14. It begins with the Ice Age and the First Men, and ends with the eighteenth century A.D. The first 40 pages deal with primitive man; then follow 34 pages on the classical civilization of Greece and Rome; the remaining 106 pages are devoted to the Dark Ages and modern times.

There is one obvious major criticism of this scheme; it excludes the Ancient East where civilization arose. It was in the East that big accumulations of capital can first be recognized historically—in Mesopotamia in the form of temple store-houses, in both Mesopotamia and Egypt in irrigation works and ships. Now it is in accordance with the Marxian, materialist, conception of history that the main path of human progress has hitherto been through the accumulation of the means of production (Marx's 'instruments of labour') into a few hands. For instance, an irrigation system is impossible without unified control. But there is no mention of these early civilizations in the book. We pass from palaeolithic man to the lake-village, as if the latter were an essential link in the chain of progress, which is historically incorrect. The first 'productive economy' of Europe was introduced by the Danubians, who acquired it elsewhere. It was in the East, not in Europe, that human society evolved; and western Europe was until much later merely a marginal region of refugees or at the most of barbarians off the main line of the cultural evolution of humanity.

Religion is dealt with in summary fashion on Marxian lines. An opportunity

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is missed of emphasizing the futility of the Cult of the Dead, which assumed such great proportions during the megalithic period in Western Europe, to say nothing of the Pyramids. The treatment appears to be ethnographic rather than historical.

Throughout the book emphasis is rightly given to the economic basis of society, and to the life and conditions of the workers. There is great need for human history to be thus rewritten on Marxian lines, and so far as we are aware no more ambitious attempt has been made to achieve this. In Marx's time it would have been impossible, for then whole epochs which the spade has revealed were completely unknown. No doubt the need will in due course produce the man and the book, or books. Meanwhile the younger generation will have to be content with such imperfect but by no means useless productions as the present. O.G.S.C.

THE ARCHAEOLOGY OF SOMERSET. By DINA PORTWAY DOBSON. *Methuen*, 1931. pp. 272, 58 illus. and 7 maps. 10s 6d.

Mrs Dobson's book provides a welcome and exhaustive guide to the archaeology of Somerset and is a notable addition to Mr Kendrick's series of county archaeologies.

Somerset does not lend itself particularly well to treatment as an archaeological province. It corresponds to no clearly defined geographical region, but contains within its borders areas as different in their relation to human occupation as the limestone plateau of Mendip, the alluvial plain of the Somerset Fens, and the sandstone moorlands of the Quantocks, Brendons, and Exmoor. This being the case it is inevitable that Mrs Dobson has been obliged to arrange and present her material in the form of a catalogue of archaeological discoveries, epoch by epoch, prefacing each chapter by a short consideration of the special characteristics of each culture in Britain and the way in which its influences are likely to have impinged on the county. In this enterprise she has succeeded, but the distribution maps, so important in a book of this kind, leave much to be desired. The maps are not bad in themselves, but their reproduction is far too small and the crowd of contour lines made necessary by the hilly nature of much of the country makes it very difficult to track down some of the symbols. This trouble clearly arises from a question of cost, and it is a pity that Messrs. Methuen have permitted yet another of this series to suffer from this defect. Good illustrations and clear maps are prime necessities in archaeological publications and colour printing with better paper would greatly improve the maps in this volume.

The first chapter deals admirably with the geography and geology of the county and is a model of how this matter should be handled on a small scale. There follows a chapter on the Palaeolithic Age, one surprisingly named 'Beakers and Megaliths', and so on through the Bronze, Early Iron, Roman, and Saxon Ages to complete the book with two chapters on camps, earthworks, and trackways. There is also a good archaeological gazetteer.

The richness of Somerset in Upper Palaeolithic remains is well-known and receives ample treatment in this book. Mrs Dobson has also made out her case for the 'Beakers and Megaliths' title on the local evidence, though she has been inclined not to recognize the comparative lateness of the stone circles as compared to the megalithic tombs.

An interesting development in Somerset prehistory which has begun to take shape too late for inclusion in the book under notice is the growing volume of evidence accumulating as a result of the local work of the Bristol University Spelaeological Society, showing that Mesolithic flint-working technique survived in Somerset to overlap with that of the early Metal Age. In this Somerset is not alone.

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Leaving the Bronze Age, the later stages of which are not very clear in Somerset, we come to the Early Iron Age which is particularly well-represented by the lake villages of Glastonbury and Meare, Wookey Hole, famous hoards, and several of the finest hill strongholds in Britain. For the Roman occupation there is Haverfield's article in the Victoria County History which has lightened labour in a county which is embarrassingly full of Roman material.

The Saxon chapter contains a valuable historical account of the vicissitudes of the region at this period, but has little archaeological material to deal with before the opening of the Christian period. In spite of Glastonbury the county is poorer in surviving Anglo-Saxon remains than would be expected. The Alfred Jewel, perhaps the county's greatest archaeological treasure, receives due attention, but it is difficult to reconcile plate vi where the jewel is said to be represented at full size with the statement on p. 184 that it is 6.3 inches in total length.

In the chapter on earthworks there is a surprising slip in the account of Wansdyke. It is said on page 222 that the age of the dyke was proved by excavation in Wiltshire by Professor Haverfield, whereas this famous test excavation was carried out by General Pitt-Rivers. Again, it is impossible to concur with the view of the late Mr Albany Major that the Wansdyke began on the Severn shore by Portishead and made its way across the Failand Ridge, through Ashton Park, to join the generally accepted part of the dyke at Maesknoll on Dundry. The few traces which Mr Major considered to represent its line in this area are either of no great antiquity or are parts of Celtic field-systems, which are very prolific here. This view is supported by the experience in the field of Mr O. G. S. Crawford.

There is also a mistake on p. 206 when it is claimed that the circular earthwork called Old Barrow overlooking the sea near Lynton is a sepulchral monument. It is fairly certain that this is a Roman signal-station, and it is one of the tasks before Devon and Somerset field-workers to find the sites of others which may reasonably be assumed to exist.

A last point. It is a pity that the sketch of the bronze anvil recently found at Flax Bourton should be so placed on the dust cover of the book as to look like the portrait of an excessively cadaverous person with a long nose putting out his tongue at the purchaser! Surely this should have been shown with the body of the anvil horizontal and the holding spikes pointing down?

This book is strongly recommended as a guide to the present state of the archaeology of Somerset and Mrs Dobson is to be congratulated on the completion of a valuable piece of work.

FARA. *Ergebnisse der Ausgrabungen der Deutschen Orient-Gesellschaft in Fara und Abu Hatab 1902-3* von ERNST HEINRICH. Herausgegeben von WALTER ANDRAE. *Vorderasiatische Abteilung der Staatlichen Museen, Berlin*, 1931. pp. vii, 156, with 69 figs. and 78 plates. 90 Rmk.

For many years Fara has figured prominently in all discussions of the Early Dynastic Period in Sumer. For the large collection of archaic cuneiform tablets found there supplies the earliest information as to Sumerian culture, and all attempts to solve the chronology of the finds from Ur and Kish have been based on them. In this well-produced volume we are for the first time enabled to review the material culture of the writers of the Fara tablets, and to compare it with that known from other

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Sumerian sites—from Tello, Assur, Kish, Ur and Khafaje. Unfortunately the outstanding question, namely which precisely of the objects here figured are, for certain, contemporaneous with the tablets, cannot be answered as definitely as one should wish; for very little is known of the circumstances in which each object was found. The authors are acutely aware of this, and appropriately introduce the volume with a reminder that the work at Fara, undertaken 30 years ago, was rather in the nature of an archaeological reconnaissance, a mere episode in the long campaign of pre-war German excavations in Mesopotamia, and that, of its personnel, Koldewey soon became completely absorbed by Babylon and Andrae by Assur. All the more do they deserve our gratitude for not shirking the task of working up old and admittedly unsatisfactory notes while a variety of circumstances provided ample excuse for dropping the matter altogether. But fortunately the very method in which the investigation of Fara was carried out counteracts the effects of insufficient recording to some extent. For Koldewey's uniform trenches touched almost exclusively one particular stratum, as the homogeneity of the material contained in this volume shows, and the links with all the other sites where Early Dynastic remains are found, and which we have enumerated above, are numerous and convincing. If, therefore, we cannot expect any help from this collection of material when at last the subdivision of the Early Dynastic Period itself is undertaken, the danger of ascribing to that period as a whole objects which are earlier or later, is not great. But there are some terracotta plaques (pl. 25) which might really belong to the period of the Third Dynasty of Ur, while at least one seal-cylinder is definitely Akkadian (pl. 59 c). On the other hand some of the seal-cylinders and seal-impressions may well be older than the bulk of the material (e.g., the pot illustrated on pl. 18, i, seems to belong to the Uruk Period). Then there are some fine polychrome jars, here adequately published in colour (pls. 14-16) which are in shape and design typical representatives of the Jemdet Nasr period.* We should, in fact, refer here to the very interesting investigation which Dr Erich Schmidt made at Fara during the winter of 1931 on behalf of the University Museum of Pennsylvania (*Museum Journal*, xxii, 193 ff.), with the express purpose of elucidating the stratification. He went down to virgin soil and found that the site was only inhabited after the Jemdet Nasr period, just as Kish was, or at least the region of Kish touched by M. Watelin's deep shafts. He also discovered another fact which explains why the German finds at Fara are so homogeneous: the Early Dynastic strata were separated from the earlier Jemdet Nasr culture by a sterile alluvial deposit. We may add that a similar layer was observed in exactly the same position at Warka, and these deposits (if we *must* find a historical reality underlying myths) have a better claim to represent the Flood of Sumerian tradition than those at Ur or at Kish, the first of which comes too early, and the other too late in the history of human settlement in the plain to accommodate the prediluvian hegemonies of various cities. But to return to the new information obtained by Dr Schmidt, the date of the large circular cellars or granaries of planoconvex bricks, which was uncertain in 1902, is now established as Early Dynastic; while the fact that they were reused as a burial-place in the time of the third Dynasty of Ur makes it less imperative to consider the inscribed cone discussed in the German volume and also belonging to that period (p. 4f) as an

* A pot like, e.g. *Corpus Vasorum II*, pl. 10, 3 supplies the type of the sherds, pl. 14, i and 1 which seems to worry Herr Heinrich on p. 35.

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importation ; and as this cone is the only piece of evidence for identifying Fara with the ancient Shuruppak, this matter is of some consequence.

We may now summarize the contents of the book as far as it falls outside the scope of the more general remarks which precede. It opens with a short description of the campaign by Andrae, which is interesting because we are always apt to forget that the Ford car is revolutionizing the formerly unchanging East at such a rate that it is already difficult to recall the difficulties and privations (but also the long periods of quiet and the absence of hurry) which pre-war excavators experienced. Heinrich next discusses the architectural material, including a most interesting group which he explains (p. 14) as an office of the period. It consists of three connected rooms, situated immediately behind the entrance of a house, but completely separated from the living quarters. If we are astonished to find them equipped with a hearth and a water-drain, we may remember that water was necessary to soften the tablet-clay and fire sometimes to harden it. And as a matter of fact, numerous tablets were found in these three rooms.

If it is impossible to refer specifically to the numerous finds now published for the first time, we must in any case draw attention to a set of artist's sketches on clay (pls. 27-32). There are also glass beads, and an analysis of these is given ; they would provide a first class sensation if only the experts would declare that they were made by man and are not natural glass. But the experts (and all whom the reviewer has known to be called in by archaeologists) do not commit themselves (p. 79). There are two analyses of tools (p. 89) which contained 97 per cent. copper, and for the rest mostly arsenicum and nickel. This tallies with the analysis of the copper from Ur and Kish, by Professor Desch, but not of that from Khafaje which is contemporaneous ; and it may be that the region south of modern Baghdad obtained its copper from Oman, but not the more northern regions (see ANTIQUITY, II, 452 ; is this article by Mr Peake perhaps the source from which the periodical UMSCHAU, in the same year 1928, got its strangely anonymous information ?).

The collection of seals and seal-impressions is carefully studied and too rich for any detailed comment to be possible here. The grouping of the seals according to their representations is particularly appropriate in a collection which is of one period ; for it brings together designs which elucidate one another ; and some of them only then become intelligible. The last chapter of the book gives an account of the short exploration of the neighbouring Abu Hatab, where the remains proved to be contemporaneous with Ur III.

This book apparently represents an attempt to reduce the cost of scientific publications by eliminating publishers as agents. This is suggested if one compares its price with that of similar publications of the Deutsche Orient Gesellschaft. Let us hope that the courage of Dr Andrae in issuing it through his own department of the Berlin Museum will be rewarded by a sale commensurate with the high scientific value of the material which it contains.

H. FRANKFORT.

AN ARCHAEOLOGICAL TOUR IN GEDROSIA. By SIR AUREL STEIN. Memoirs of the Archaeological Survey of India, no. 43. Calcutta, 1931. pp. 211 with 33 maps, 13 plates and 64 figures. 32s 6d.

Sir Aurel Stein's tour through eastern and southern Baluchistan forms only a small part of his plans for further exploration and his report may be considered as a collection of material. Naturally it takes the shape of a travel-journal, but with

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this peculiarity that the account of the author's progress is made subsidiary to the discussions of ancient remains which were surveyed, and of trial excavations which were undertaken at a few points when time and circumstances permitted. It is simply amazing that it should have been possible in little more than four months to cover an area of some 270 miles from north to south and 300 miles from east to west; and to bring home so rich a harvest. Its full value will only be seen when at two or three points within this territory excavations are undertaken on the lines of Mr Hargreaves' exemplary work at Nāl. Then, no doubt, the finds published in this volume will fall into place and the whole of the region which Sir Aurel Stein succeeded in exploring by dint of much hard travel will be incorporated in our picture of the ancient world. For the moment the following conclusions are suggested.

Baluchistan was more densely populated and more intensely cultivated in chalcolithic times than in any subsequent period. There is now a population of only 2 persons per square mile (Sir Aurel Stein found difficulty in getting help for investigating mounds). The remains of ancient settlements are found in considerable numbers along river-beds which are only rarely in spate nowadays. Sir Aurel repeatedly insists on the difference in climate which must, without a doubt, explain the mere existence of the ancient settlements (pp. 34, 106, 131-2 and elsewhere). It may be recalled that Sir John Marshall (*Mohenjo-daro*, 1, 2-5) attributes the change not so much to reduced winter-rain as to a withdrawal of the monsoon. This same authority has also studied the pottery from Baluchistan (*Mohenjo-daro*, 1, 96-100) and this is of exceptional importance, for it appears now that those features of the Baluchistan pottery which are most inexplicable to the student who approaches it from the west (for instance, the prevalence of red body-colour and a number of designs) find their parallel in the Indus civilization. Sir John actually considers the Baluchi wares as mixtures in which Indian and Persian elements are present in varying degrees. A study of the plates in Sir Aurel Stein's volume leaves no doubt as to the identity of the Persian elements. They belong to that Iranian Highland culture, which is best known from Susa 1, which sent off-shoots into Mesopotamia as soon as the Plain became habitable (as the discoveries at al'Ubaid, Ur, Warka, Samarra and other places prove) but which is now also traceable throughout Persia, at Persepolis, Hekatompylos, Mohammedabad and eastwards into Seistan.* We do not maintain that all the Chalcolithic Baluchistan pottery which we actually possess now is of that very early date. On the contrary, some of it Sir John Marshall thinks to be contemporaneous with the Indus civilization, of which the only stage so far discovered is shown by the finds at Tell Asmar to be contemporaneous with the Dynasty of Akkad in Babylonia (*The Times*, 26 March). But even then the western elements surviving in the Baluchistan pottery of the middle of the third millennium must go back to the much earlier Iranian Highland culture. This is shown in the curious way in which all the stages of Baluchistan civilization discovered by Sir Aurel Stein are individually linked up with the Iranian Highland culture, independent of the differences which they show amongst themselves. He distinguishes a first stage represented by a very fine pottery for practical use, known from Suktagēn-Dōr, Mehi, Kulli and other sites, and showing, amongst other decoration, rows of goats or ibexes identical with those of the disintegrating Susa 1 style

* This whole matter is treated in detail by the reviewer in his *Archaeology and the Sumerian problem*, University of Chicago Press, Chicago 1932, where he endeavours to show that these earliest settlers in Mesopotamia are the Sumerians.

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found at Tepe Khazineh and Tepe Musyan. A later stage of civilization in Baluchistan is known from graves which were dug in Shahi Tump after buildings (of mud brick walls on stone foundations) belonging to the first period had been deserted. On the other hand this later stage uses pottery, which Sir Aurel Stein believes to be made exclusively for funerary use (pp. 98, 101) and to retain obsolete traditions (p. 103). This pottery is indubitably linked up with Susa I by its trio of typical shapes (straight-sided beaker, hemispherical bowl and squat pot) as well as by its decoration. That there are some links with earlier Baluchistan wares Sir Aurel Stein also admits (p. 70). Nāl, discussed in *ANTIQUITY*, 1930, 396 ff., stands rather apart but enters, if somewhat more distantly, into the same group, appearing as an independent and advanced development founded on a basis common to the whole Iranian Highland culture. Sir Aurel Stein thinks it to be contemporaneous with the tombs of Shahi Tump (p. 103). Sir John Marshall, on the other hand, considers its makers intrusive in eastern Baluchistan, stresses the relations with the west and thinks it earlier than anything found at Mohenjo-daro and Harappa; which means, by implication, that it would also be earlier than a number of red-faced Baluchi wares related with the Indus wares (*Mohenjo-daro*, I, p. 100 with notes). He also mentions some new evidence obtained by Mr. N. G. Majumdar in Lower Sind, which perhaps goes to show that light-faced fabrics such as those from Nāl and from the Iranian Highland culture in general precede the red-faced fabrics. Should this prove correct then we should have to assume that not only in Baluchistan but even in Sind the Iranian Highland culture preceded that which is revealed by the excavations, and that it descended into the plains beyond its eastern mountain ranges just as it colonized the Plain of the Two Rivers.

It is interesting that even in the earliest known stage of civilization in Baluchistan copper was well established in use, so that not only ornaments and weapons, such as flat axes and spearheads, but also mirrors, were made of it, as at Susa I. Still more unexpected is the discovery of a piece of glass, part of a ring or of a handle, which was found deep down in Shahi Tump (p. 94). Terracotta figurines of goddesses and humped bulls abound. Twice, once in a grave and once in presumably earlier layers, a copper object of the type commonly called stamp-seal was found (pp. 92, 98).

As to the modes of interment three different methods are found in use, in Baluchistan as well as in India, in chalcolithic times:—(1) complete burial, (2) fractional burial, in which only a fraction of the bones are collected and buried after the body had been exposed to beasts and birds, and (3) burial of what remained of the body after cremation. Sir John Marshall (*Mohenjo-daro*, I, 79-90) comes to the conclusion that the post-cremation burials are typical for the Indus civilization, and that the other two types are due to other (western) racial groups remaining faithful to their own funerary customs. This may well be so. But in addition we should observe that in Baluchistan not much importance seems to have been attached to strict rules in matters of interment; even within the range of one single type of burial variations occur. With the complete interments, the position of the body in the grave is sometimes contracted, sometimes supine and extended (p. 95). In the post-cremation burials we find sometimes the remains of the body gathered after cremation and buried in a pot, sometimes the remains are left in the place where the cremation had taken place and the funerary gifts are placed round them (pp. 155, 157-8, 163). At Nāl Mr Hargreaves found fractional and complete burials which were apparently contemporaneous. It seems to us that the absence of strict funerary rules is characteristic

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for the Iranian Highland culture : for in the west we find within its scope the same variety. At Susa we find fractional burials in pots, but also complete burials in contracted positions, while in the related graves at Ur, of the al'Ubaid period, we find the complete burial in supine extended position.

Two complete skulls and some bones were saved, and are studied by Lt.-Col. R. B. Seymour-Sewell and Dr B. S. Guha in an appendix ; the people, like those of Nāl and Anau, were dolichocephalic.

Finally it may be stated explicitly that the new material discovered by Sir Aurel Stein, and that found in the Indus Valley, cannot be expected to reach its maximum of scientific utility until a representative collection has been sent on exhibition to this country for archaeologists to study. It is an urgent problem to determine what connexions existed between the new civilizations discovered in the east, and those of Egypt and Babylon from which we consider our own to be directly descended. If questions of contact between civilizations have to be decided on the testimony of material remains alone, technical qualities and processes of manufacture gain a unique importance ; and no opinion on any of these matters can be formed merely from reproductions, however adequate these may be.

H. FRANKFORT.

LE FEU ET L'INDUSTRIE DE PIERRE ET D'OS DANS LE GISEMENT DU 'SINANTHROPUS' A CHOU-KOU-TIEN. By H. BREUIL. *L'Anthropologie*, 1932, XLII, 1-17.

LE FEU ET L'INDUSTRIE LITHIQUE ET OSSEUSE A CHOU-KOU-TIEN. By H. BREUIL. *Bulletin of the Geological Society of China (Peiping)*, 1931, XI, 147-54.

NOTICE OF THE DISCOVERY OF QUARTZ AND OTHER STONE ARTIFACTS IN THE LOWER PLEISTOCENE HOMINID-BEARING SEDIMENTS OF THE CHOUKOUTIEN CAVE DEPOSIT. By W. C. PEI. *Ib. id.* 109-42.

The discovery of fire and the invention of tools were two of the earliest and most epoch-making discoveries of human history. Much legitimate but necessarily inconclusive speculation has been made about the earliest sub-human and human efforts. Here at last in China we have got some facts. What is more, we have got 'man and his works' associated in the same deposit. It is too often forgotten that we really know almost nothing of the bodily form of the *coup-de-poing* makers of Chellean and Acheulean times ; and *coups-de-poing* are the commonest form of palaeolithic object in this country, to be found in hundreds of gravel-pits all over the London and Hampshire basins.

Mr Pei has given us a careful account of the objects themselves—illustrated by drawings and photographs. The implements are of quartz—a peculiarly resistant medium to the knapper and also, it would seem, to the photographer. And here we must interpose a few general remarks about the photographic illustration of prehistoric objects. These illustrations are neither better nor worse, in our opinion, than many others of their kind in scientific proceedings. The background is cut away, however, and the general impression left is a blurred and unsatisfactory one. These defects are to some extent remedied by the drawings, which cannot however be compared with those of some French and English prehistorians. We realize that studio finesse

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may not be compatible with rapid publication 'in the field'; what we regret is that so many really expert amateur photographers in Europe are wasting time and money over precious studies of bunches of grapes and the like, when here is a real technical problem to be tackled. Here are hundreds of the oldest implements of humanity, implements which necessarily can only be seen by a tiny fraction even of specialists. Their cultural import is immense; but it can only be realized by the majority of students through photography—we doubt whether even casts would be adequate. We should like to see a few type-specimens subjected to the expert studio treatment of a skilled artist-photographer, who would experiment with lighting and background, and publish the results *de luxe*. Such a task is more worth while than many of the publications *de luxe*—some of them archaeological—to which the world is treated.

However, we hasten to return and assure Mr Pei that we fully realize the impossibility of such elaborate treatment in this instance; and our criticisms may therefore be irrelevant in this particular case. Early publication is always more important than *finesse*; and both he and Dr Davidson Black have consistently followed an irreproachable and commendably scientific policy both in the style and promptitude of their publication, and, it may be added, in their attitude to the gutter press.

The implements, so far as one can judge without seeing them, seem to be extremely rude and to consist for the most part of 'flakes', using the word to connote 'irregularly shaped pieces which have evidently been formed by fracture from some larger stone or fragment' (p. 122). Professor Breuil hints at the possibility of both the man and industry of Chou Kou Tien being ancestral to our Mousterian. Of the human origin of the industry there can be no doubt. The stone of which they are made is foreign to the immediate locality, and can only have got there by human agency. This acute observation was first made ten years ago by Dr J. G. Andersson, five years before the world-renowned skeletal remains were found; and indeed their discovery was a confirmation of his brilliant reasoning. The implements do not therefore suffer from the defect which necessarily handicaps stones claimed to be artifacts but found in deposits that have never yielded skeletal remains of man. This defect may, of course, be merely accidental, but it remains a defect.

It is not, however, necessary to rely only upon worked stones for the proof that *Sinanthropus* was a tool-maker. 'The workman of Chou Kou Tien', says Professor Breuil (p. 11), 'made extensive and systematic use of bone, deer's horn (*ramure des Cervidés*) and the horn of ruminants of small size (*Gazelles et Spirocères*)'. No illustrations of these are as yet available. Implements were made by cutting the horn with a stone gouge, the point selected for cutting having first been burnt. The piece was finally severed by breaking. The result consisted of objects resembling clubs and daggers. Out of the frontal bones of deer were made cups, by trimming the edges. Some of these have that part of the lip which could be placed against the mouth polished by use (Breuil, p. 11). The long bones of animals were also used, the articular head forming a convenient natural handle; the other end was shaped to a point or edge which often bears traces of usage.

Such in briefest outline is the 'industry of Chou Kou Tien', baldly summarized from the masterly account of Professor Breuil. He concludes with some remarks which we can only refer to here by the statement of points.

1. 'The idea of forcibly taking from animals the weapons which Nature has given them is, when one reflects upon it, a more primitive one than that of fashioning

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a stone. It is quite possible that an age of horn (*bois*) and broken bone preceded the age of worked stone, as more than one prehistorian has thought'.

2. The implements found are the work of *Sinanthropus* and not of some other type of man.

3. 'The existence in so primitive a skull, one so remote from that of all other known types of mankind, of a human aptitude for inventing tools and implements designed to make others, does indeed open up an unexpected vista of the most distant past of our species'.
O.G.S.C.

THE MAP OF ENGLAND, or About England with an Ordnance Map. By Colonel SIR CHARLES CLOSE. London: Peter Davies, 30 Henrietta Street, W.C., 2, 1932. pp. x, 166, 8 illus. 6s.

No one is better qualified to describe the map of England than Sir Charles Close, the author of the principal text-book on Topographical Surveying, a former Director General of the Ordnance Survey and President of the Royal Geographical Society. But it would have been quite possible to have all these qualifications and yet produce an unreadable book. The present book is eminently readable—an example of a 'popular but authoritative' production that will also, we hope, have a 'world-wide circulation'. It was time for such a book, indicating the inexhaustible stores of history that lie dormant on an Ordnance Map (particularly the 6-inch), ready to wake to life at the touch of a lively but informed imagination.

The first chapter is devoted to Old Maps of England, and the author, with true insight into primitive mentality, rightly says that we may 'take it for granted that the idea of a map existed in this country for many centuries before the coming of the Romans'. The idea of a map (and even ephemeral sand-drawn or other scratched sketch-plans) exists wherever there exist also people familiar with a bit of country; and the prehistoric builders of Stonehenge and Avebury with their avenues, to say nothing of the later defences of the Verulamium district, must have drawn many such. More familiar ground is traversed in describing the maps from Ptolemy to Saxton and Cary; but we miss the anonymous author of the Peutinger Tables—the nearest approach to a contemporary map of Roman Britain.

The Ordnance Survey has, as it deserves, a couple of chapters to itself. Then comes one on 'How to read a map, and some other matters', from the first part of which 'accomplished and well-informed people are warned off'! (We once however, met one such who believed us when we told him, in a moment of temptation, that certain parallel lines on a hill-slope were contour-lines revealed by the magic of air-photography). The 'other matters' include a statement which deserves the widest publicity, that 'it is impossible thoroughly to study the surface-forms, the soils or the archaeology of a district, without paying attention to its geology'.

The chapter on Place-names is based on the authoritative work of specialists and is refreshingly sane, as one would expect from the President of the English Place-name Society.

Prehistoric England has a chapter to itself, specially to be commended to those enthusiasts who may be thinking of writing long letters to experts; they will find many of their difficulties solved here. Sir Charles quotes, and improves, 'Thurnam's dictum' about long and round barrows on p. 90 ('Long barrows were made a long time ago, that

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is they are older than the round barrows, and they are the resting-places of *long-headed chiefs* ; we would however, suggest 'people', for the graves are communal).

The author professes a 'half instinctive repugnance to the Roman period' ; but he has successfully disguised it, and his remarks on Roman roads are full of practical wisdom. It is good to find the element of conscious, deliberate planning of a road-system brought out, and to be told that the Roman engineer ignored existing trackways, as he undoubtedly did. How much rubbish has been written in the contrary belief can be appreciated only by the unwilling recipients of it.

The two concluding chapters deal with Saxon and Elizabethan and with seventeenth century England respectively. The Battles of Cheriton and Marston Moor are described with reference to the 6-inch maps, where their development can still be followed. The last pages lead up to a quotation from Bunyan's *Pilgrim's Progress* that may be commended to the Publicity Department of the Ordnance Survey.

An admirable feature of the book is the list of books, maps and articles at the end of each chapter—not citations of cheap hackneyed stuff but of the work of real first-hand students. Every reader of ANTIQUITY should buy this book, if it is not already out of print. O.G.S.C.

THE ARCHAEOLOGY OF CORNWALL AND SCILLY. By H. O'NEILL HENCKEN, PH.D., F.S.A. SCOT. Assistant Curator of European Archaeology, Harvard University. County Archaeologies: General Editor, T. D. Kendrick. *Methuen*, 1932. pp. XVIII, 340, with 9 maps and 59 illustrations. 10s 6d.

The volume on Cornwall possesses one initial advantage that is denied to others of this series. By its geographical position, and circumstances largely due to that position, the county forms an entity in a sense that no other English county does. It can, therefore, be more logically dealt with as a separate archaeological region than counties whose boundaries have no meaning in relation to conditions in prehistoric times. In the performance of his congenial task Mr Hencken has fully grasped the opportunities this offered to him. The book should appeal not only to those interested in Cornish antiquities, but to all those concerned with the study of British archaeology. The illustrations and maps are a distinct improvement on those of some of the previous volumes of this series.

With the necessary modifications the plan of the book follows that of its fellows. The Palaeolithic period, almost unrepresented in Cornwall, is dealt with in a single page, thus leaving more space for the consideration of 'Great Stone Monuments', of paramount importance in the region. These include chambered tombs, circles, and standing stones. As in Brittany the megalithic remains of the Isles of Scilly seem out of all proportion to the size and fertility of the land ; what the author justly describes as the 'amazing number' of chambered tombs (there are still 43 in the Isles) gives rise to a cautious speculation as to the possible reasons for it. It is recognized that some, if not all the chambered barrows of the Isles, as well as of the mainland, were made by people acquainted with metal, a piece of bronze or copper having been found in an undisturbed chamber on one of the islands, and whetstones, indicative of the use of metal, in two instances on the mainland. The statement that all British megalithic tombs were originally covered by mounds is perhaps a little too sweeping.

The bluish green segmented beads, for which an Egyptian or Cretan origin has been claimed, have been found in several burials in Cornwall. While claiming that the presence of these beads PROVE trading with the eastern Mediterranean in the Bronze Age, the author says that some were probably made locally. In view of the fact that this

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is one of the simplest forms of bead to make, and turns up in many widely separated regions and dates, and that no other objects from the same foreign source are known in Britain, is it not at least probable that all these beads are of local origin, as seems to have been the case with similar beads found in Scotland?

Perhaps the most interesting chapter in the book is that on the prehistoric tin trade. It should dispel once and for all the myth of Phoenician trade with this country.

The volume includes an 'Archaeological Gazetteer' for the county, and lists of collections and museums in which Cornish objects may be found, of megalithic monuments, and of bishops of Cornwall. There is a good index. M. E. CUNNINGTON.

REPORT ON EXCAVATIONS AT JEMDET-NASR, IRAQ. By E. MACKAY, with preface by STEPHEN LANGDON. Field Museum of Natural History, Anthropology Memoirs, vol. I, no. 3. *Field Museum of Natural History, Chicago, 1931.* pp. 75 and 18 plates. Price not stated.

Jemdet-Nasr lies about 15 miles northeast of Kish (Tell Uhaimir), which in turn is 8 miles from Babylon. This means that we are in the heart of Babylonia, called in ancient times the Land of Akkad. The site of Jemdet-Nasr was deserted at a remote period, well before the beginning of the historic period beginning about 3100 B.C.; consequently all that is found there bears the marks of a homogeneous civilization datable to about 3500 B.C. Amongst the ruins of a large building of thin flat bricks (whose exact nature remains undetermined), the expedition found inscribed tablets; these will form the subject of another monograph by Dr Langdon. The signs on them were still almost pictographic, that is to say, they resemble the objects they are meant to represent, and have not yet acquired a cuneiform appearance. With these tablets were found many pot-fragments of two kinds;—those which are unpainted have the form of jugs with obliquely set spouts or slightly curved like the spout of a tea-pot; the others, shaped like flat water-pots, have high handles whose curve rises well above the vessel itself. The remainder are 'craters' with small perforated lugs set round them for suspension. All these forms are recorded from other Mesopotamian sites, but only at the beginning of historical times. In addition to these unpainted vessels Mr Mackay found numerous painted ones, some with a single colour (black), some in several colours (black and two shades of red). The patterns are geometric, laid on in broad lines. They consist of animal and vegetable designs. The general appearance of this pottery is rather coarse, the commonest shapes being the saucepan or jar; many specimens have perforated lugs like the unpainted ones. Jemdet-Nasr has yielded stone and metal implements and cylindrical envelopes for tablets of archaic character; but no graves have been found as yet.

The polychrome painted pottery of Jemdet-Nasr is of a kind unique up to now in Mesopotamia (the monochrome pottery found at Eridu, Ur and many other places being older and quite different in character). It can only be compared with the pottery found in the mounds which occur round Musyan, about 95 miles west of Susa in Persia. This pottery is later than that from the deep strata at Susa, but is yet earlier than the beginning of history in Lower Mesopotamia, when painted pottery had ceased to exist. The great value of the excavation of Jemdet-Nasr, well conducted and well published, is that it states the problem of the relations then existing between the civilization of central Mesopotamia and that of ancient Persia. G. CONTENAU.*

* This, and the following review by M. Contenau, are translated by the EDITOR.

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NEW LIGHT FROM ARMAGEDDON; Second Provisional Report (1927-9) on the Excavations at Megiddo in Palestine. By P. L. O. GUY. [Oriental Inst. of Chicago, Communications, no. 9]. Univ. of Chicago Press. pp. 68, and 61 illus. 1 dollar.

Armageddon is a corruption of Har Mageddon, the hill of battles. The modern form is Megiddo and here are the excavations being directed by Mr Guy, whose second preliminary report, covering the campaigns of 1927-9, lies before us. The site of Megiddo* is a hill, today called Tell-el-Mutesellim, rising abruptly from the plain of Esdraelon; its position has given it strategic importance in all periods. The excavations have reached the archaeological stratum described as IV, and have brought to light both tombs of the Middle Bronze Age and architectural remains of the old town; in particular the town gate, which though simpler in plan closely resembles the south gate of Carchemish on the Euphrates, and is a good example of the great differences between Syrian and Assyrian gates, the latter amassing even more means of defence than the former. The street paving was laid bare, and the lower parts of certain houses, whose construction, in the case of important buildings, consisted of stones of unequal size, with pillars of dressed stone set at intervals, some having masons' marks like the famous Solomon's Seal. One of the most remarkable quarters of the town consists of great stables; the horses were put in two rows divided by a central corridor; the roof was supported by pillars which also served to mark off the space allotted to each animal. These stables could contain a large number of horses (120 under a single roof); they give one an idea of the importance of war-chariots at the period in question (about 1000 B.C.). Palestinian sites are usually rather poor in objects; we must be grateful to Mr Guy for carrying out his investigations in such a way as to provide a correct interpretation of these Megiddo buildings, whose analogues, frequently met with in Palestine, had never been satisfactorily explained. The scarab, described by Mr Staples, has a winged Egyptian griffin and a *crux ansata*, which shows Egyptian influence; but it also has a grasshopper, suggesting Mesopotamia. The griffin was used by artists everywhere in the Near East, but its prototype occurred already on the Egyptian palettes (Hieraconpolis). Mr Staples cites many passages where Egyptians, Assyrians and Hebrews compare their enemies to grasshoppers, thus using this insect as a symbol for a multitude. We know besides that the grasshopper is the emblem of a Mesopotamian deity; on an enamelled brick from Assur it figures beside the god amongst the symbols of divinity surrounding it. Perhaps one should simply regard the ornament of this scarab as a reflection of the different influences which prevailed in Palestine during the first half of the first millennium before our era.

G. CONTENAU.

*An air-photograph of Megiddo is reproduced on plate XVI, facing p. 465.—EDITOR.

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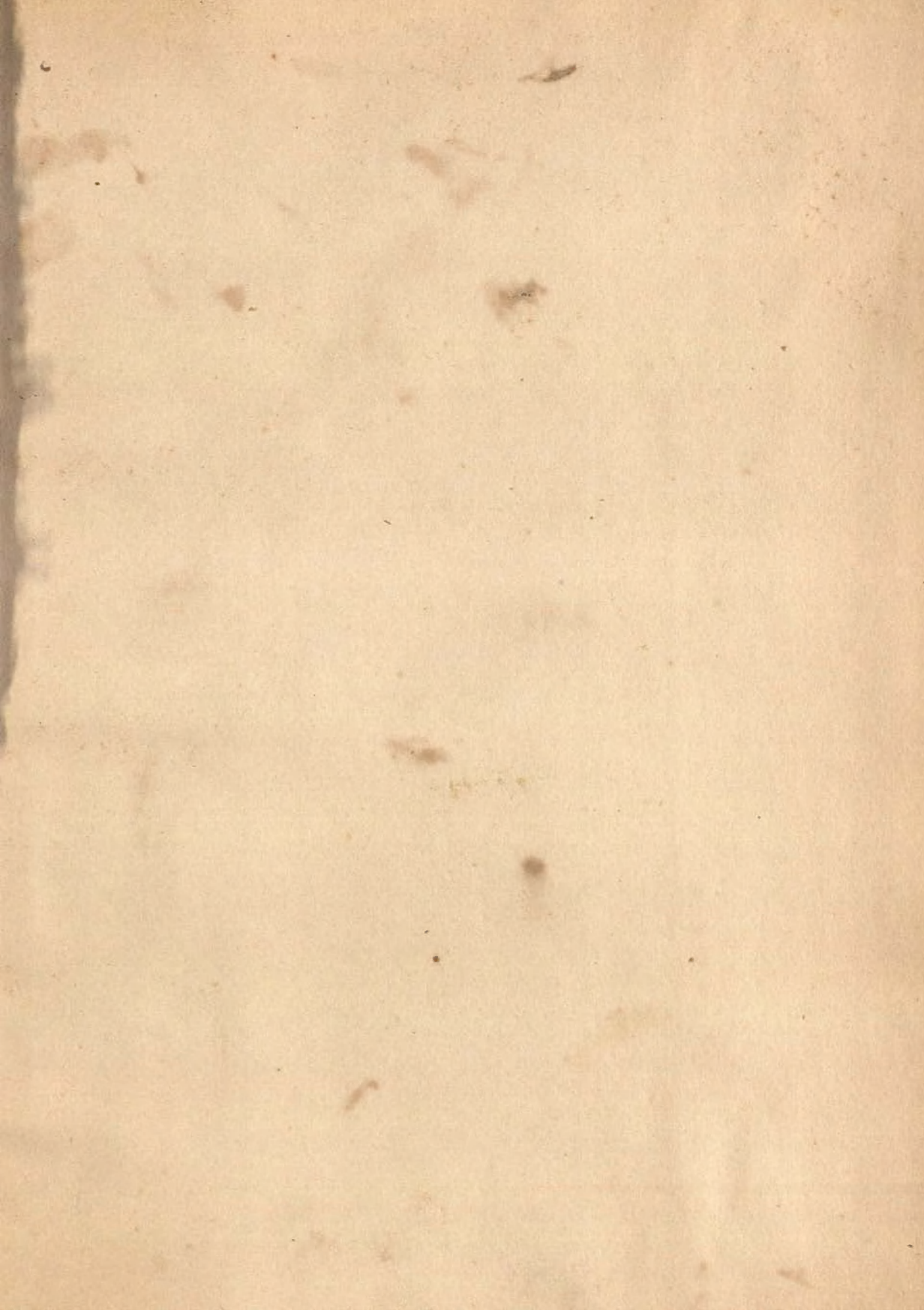
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